MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY

Vol. 104 No. 2

1944 APRIL 14

ANNUAL GENERAL MEETING

Professor E. A. Milne, President, in the Chair

The Minutes of the preceding Annual General Meeting were read and confirmed.

The President announced that the George Darwin Lecture would be delivered at the Ordinary Meeting of the Society on July 14 by Professor J. Proudman, who had chosen as his subject "The Tides of the Atlantic Ocean".

The following candidate was proposed for election as a Fellow of the Society, the name of the proposer from personal knowledge being appended:—

Elvet Lewis, R.A.F. Station, Boscombe Down, Amesbury, Wiltshire (proposed by H. H. Plaskett).

The election by the Council of the following Fellows was duly confirmed:—

Ivan Edward Allison, B.A., Dulce Domum, Peaks Lane, New Waltham, Lincolnshire;
Cyril Evans, 3 Palace Road, Llandaff, Cardiff, S. Wales;
Clarence L. Friend, El Amigo Observatorio, Casa de los Amigos, Bear Valley Road, Escondido, Cal., U.S.A.;
William Jackson, Little Ridge, Old Bursledon, Hampshire; and
Vladimir Orloff, 56 Stuart Crescent, Hayes End, Middlesex.

The Address on the award of the Gold Medal was delivered by the President (see p. 112), after which the Gold Medal was handed to Mr. Cabot Coville, First Secretary at the American Embassy, for transmission to Professor Otto Struve, Ph.D., to whom it had been awarded for his work on the observation and interpretation of the spectra of stars and nebulæ.

The President then delivered his Address entitled "On the Nature of Universal Gravitation" (see p. 120).

After the delivery of the President's Address, visitors were requested to withdraw and the Society proceeded to the formal business of the Meeting.

The Report of the Auditors was read (see p. 85).

The Secretary referred to one or two items appearing in the accounts for the year 1943.

A vote of thanks to the Auditors of the Treasurer's Accounts for 1943 was proposed and carried unanimously.

A vote of thanks to the Westminster Bank, Ltd., for their services to the Society was proposed and carried unanimously.

The Secretaries proposed, on behalf of Council, amendments to Bye-law 26A and the following amended Bye-law was put to the Meeting and carried unanimously:—

> "Notwithstanding the preceding Bye-laws, any Fellow of the Society who is on Active Service with H.M. Forces may, if he so desires, be excused from payment of his Annual Contribution until the *Council decides otherwise*. He shall not during that period forfeit his Fellowship, and his name shall not be suspended as a defaulter by reason of non-payment of such Annual Contribution, as provided in Bye-law 26; but he shall not receive the publications of the Society until he resumes the payment of his Annual Contribution.
>
> "At the end of *this* period he shall have the option of resuming his Annual Subscription from that date, or any previous date, receiving the publications of the Society from the date of resumption; or of resigning his Fellowship without any liability for arrears of Subscription beyond those outstanding at the date of his application to be excused from payment of his Annual Contribution."

The President having appointed the Scrutineers, the Society proceeded to the ballot for Officers and Council for the ensuing year. The names of those elected are given on p. 137.

The Secretary read extracts from the Annual Report of the Council.

It was proposed and carried that the Report of the Council be received and adopted, and that it be printed and circulated in the usual manner, together with the Report of the Auditors and the President's Addresses.

The thanks of the Society were given to the retiring Vice-President (Professor Brunt) and other retiring members of Council, and to Professor Chapman who retired 1943 July.

The thanks of the Meeting were given to the Scrutineers of the ballot.

The Meeting then adjourned.

REPORT OF THE COUNCIL TO THE HUNDRED AND TWENTY-FOURTH ANNUAL GENERAL MEETING OF THE SOCIETY

1. *General.*—The stabilization referred to in last year's report has been evident in the Society's affairs during the calendar year 1943. The war-time arrangements for meetings both of Council and of the Society appear to be firmly established, as is also the modified appearance and publication of *Monthly Notices.* As will be seen in the detailed report below on the various activities of the Society during the year, there has been a general, though slight, improvement on the previous year—in the attendance at meetings, in the number of books borrowed from the Library and in the number of papers submitted for publication. It is, however, the award of the Gold Medal and the George Darwin Lecture that make 1943 the most outstanding war-time year.

On two occasions during the year, the first being at the request of the Russian Academy of Sciences, the Council has given consideration to post-war scientific co-operation with particular reference to the future of the International Astronomical Union. Council has taken the view that, having regard to the fact that the International Astronomical Union is still in being and at work, post-war astronomical co-operation can best be achieved by the continuance of the I.A.U. rather than by its replacement by a new international organization.

This Report of Council follows the model laid down in 1941. It is customary to include with the Report, Obituary Notices of deceased Fellows and Associates, Proceedings of Observatories and Notes on the Progress of Astronomy. Council has decided to omit Proceedings of Observatories for 1943. Several notes on the Progress of Astronomy will be found on p. 99.

2. *Bye-laws.*—At the Annual General Meeting, on April 9, the Society adopted Bye-law 26B, which had been proposed by Council; this Bye-law extended the provisions of Bye-law 26A to Fellows who, although not necessarily on active service, were unable to continue to pay their annual contributions owing to war conditions. Entries under this Bye-law appear for the first time in the table of "Progress and Present State of the Society" on p. 73.

Later in the year the Council decided, as announced by the President on July 9, that Bye-laws 26A and 26B should be interpreted as conceding the following privileges to Fellows excused their annual contributions under these two Bye-laws:—

(i) of assuming the title of Fellow of the Society;
(ii) of attending Ordinary Meetings and Annual General Meetings of the Society;
(iii) of using the rooms of the Society and consulting the library, but not of borrowing books from the library;

but that they shall have no other privilege.

3. *Membership.*—The Council record with regret the death of one Associate (Frank

Schlesinger) and of nineteen Fellows; their deaths occurred in 1943 except where an earlier year is indicated. Obituary Notices are given on pp. 86–98.

Robert Morrison Abraham
Arthur Henry Bagnold
*Sir Sidney Burrard
Cumberland Clark (1941)
*Leonard Darwin
*William Fraser Doak
Charles Robert d'Esterre
Sir Patrick Fagan (1942)
Alfred Marshall Graeme-Thom
Samuel V. Hoffman
Lady Lockyer
Sidney Batt Mattey (1940)
*Duncan Milligan (1924)
James Edward Nicholson
Dorothea Klumpke Roberts (1942)
*Harris Charles Lindon Saunders (1931)
P. A. Scott-Iversen
*George Street
Francis John Welsh Whipple

* Life Fellow.

A table giving detailed statistics of the membership of the Society on 1944 January 1 is given on p. 73.

4. *Finance.*—The Annual Accounts have been presented in much the same form as in previous years, but in more detail for the sake of clearness.

The financial result of the year again shows a surplus, amounting to £84, 3s. 6d., after making ample reserves for amounts owing at the end of 1943. It will be noticed in the Balance Sheet that the Suspense Account and unpaid Contributions and Admission Fees amount to £808, 10s., for which no credit has been taken in the Annual Accounts.

From time to time amounts have been received from certain Fellows, who have been included in the Suspense Account, which bring their payments more up to date. Such cases have been transferred back from the non-active Fellows to the Annual Subscribers.

During the year the East India Railway 3 per cent. Stock and the India 3½ per cent. Stock held in the General Fund and Trust Funds have been compulsorily transferred to the Treasury. The proceeds of both transfers have been invested in 3 per cent. Savings Bonds, 1960/1970, in three holdings—(A) £2118, 3s. 5d. General Fund, (B) £542, 18s. 2d. Harry Watson Fund, and (C) £1122, 10s. 6d. Trust Funds (Turnor and Horrocks, Lee and Janson, and Jackson-Gwilt).

The transfer of the East Indian Railway 3 per cent. Debenture Stock into 3 per cent. Savings Bonds shows a reduction in the original stock amount (£3500–£3241, 2s. 11d.) of £258, 17s. 1d., and an appreciation of capital value on the 1942 Balance Sheet of £1368, 2s. 11d. The £542, 18s. 2d. India 3½ per cent. Stock appeared in the books at a valuation of £342, and there is therefore an appreciation of £200, 18s. 2d., making a total appreciation on the General Fund and Trust Funds of £1368, 2s. 11d., plus £200, 18s. 2d., making altogether £1569, 1s. 1d. Both these stocks were re-valued at a low figure at the end of December 1922. Notwithstanding this large increase in capital value, there will be a loss in annual income (£258, 17s. 1d. at 3 per cent. and £542, 18s. 2d. at ½ per cent.) owing to these acquisitions by the Treasury.

5. *Meetings during* 1943.—The Annual General Meeting was held in April and there were six Ordinary Meetings, the first in March and then monthly from May to October with the exception of August; this arrangement, introduced in 1941, appears well suited to war-time conditions. The average attendance of Fellows (as shown by the signatures in the Attendance Book, and excluding visitors) was 52 as compared with 49 in 1942. Meetings began at $16^h\ 30^m$ as in previous years and were timed to end at 18^h.

The May meeting was devoted to the commemoration of the four-hundredth anniversary of the death of Copernicus; at this meeting, which was attended by repre-

sentatives of Polish Institutions, Professor Herbert Dingle gave an address on the life and work of Copernicus. The George Darwin Lecture was delivered at the September meeting, and the remaining four meetings were devoted to the reading of papers communicated to the Society.

6. *Geophysical Discussions.*—No Geophysical Discussion was held during the year.

7. *George Darwin Lecture.*—Professor Svein Rosseland gave the George Darwin Lecture at the Ordinary Meeting on September 10, taking as his subject "The Pulsation Theory of Cepheid Variables". This was the first lecture given under this Foundation since the outbreak of war.

8. *Awards.*—The Council awarded the Gold Medal of the Society to Dr. (now Sir) Harold Spencer Jones, M.A., Sc.D., F.R.S., Astronomer Royal, for his determination of the solar parallax. The President's address on the award is printed in *M.N.*, **103**, 116.

9. *Publications.*—During 1943 the Society published *Monthly Notices*, Vol. **103**, *Geophysical Supplement*, Vol. **5**, No. 3, and a new *List of Fellows*.

As in 1942, the volume of *Monthly Notices* has been restricted to six numbers only, not associated with particular monthly meetings; as it is probable that publication will be continued in this form, the approximate dates of publication are given below:—

Number	Approximate Date of Closure	Approximate Date of Publication
1	March meeting	May
2	April meeting (contains Annual Report)	June
3	May meeting	July
4	July meeting	September
5	September meeting	November
6	End of year	February

The total number of pages in Vol. **103** is 354, as compared with 277 in Vol. **102** and the pre-war average of about 800; when account is taken of the increased type area and smaller type, it will be seen that the total amount of matter printed is rather over half the average.

10. *Exchange of Publications.*—The arrangements for the receipt and dispatch of publications have been very similar to those adopted in previous years, with minor modifications where changing circumstances made such necessary. The American Committee for the Continued Distribution of Astronomical Literature has again greatly assisted the Society by sending copies of some American and other publications whose distribution has been interrupted, by circulating its regular *Bulletins* and by distributing copies of *Monthly Astronomical News Letter*. The Society is now also taking advantage of the microfilm service organized by the Committee and hopes it will soon have copies of the principal papers in those publications of which only a single copy has been received. The Astronomer Royal has been able to give material assistance in the distribution of *Monthly Notices* to countries with which normal communication is impracticable. The British Council has also assisted in arranging for the transmission of publications abroad, and with the arrangement of exchanges, particularly with institutions in Russia.

The recent increase in the allocation of paper supplies for the purpose of increasing circulation will enable the Society to add to the stocks of publications reserved for

distribution after the war to those Observatories and Institutions with which communication is at present impossible.

The list of *Exchanges* has again been omitted from the Report.

11. *The Library.*—The use made of the library continues to increase and, in 1943, 390 books were borrowed by 78 different borrowers, as compared with the figures of 349 books and 78 borrowers in 1938.

Three lists of *Additions to the Library* (other than by exchange) were issued during 1943; from these lists it will be seen that the Society has been presented with and has purchased several old books to supplement its collection of early astronomical literature. The thanks of the Society are due to Mr. W. H. Owston for his generosity in presenting £13, 15s., for the purchase of three books.

Most of the more rare and valuable books are still in Oxford.

Sir John Herschel, Bt., has informed Council of his wish to present to the Society those papers and articles belonging to Sir William Herschel and Sir John F. W. Herschel which are of especial interest to the Society. Council has gratefully accepted this gift and has made a selection from the detailed catalogues at Observatory House, Slough.

A microfilm reading machine, kindly presented to the Society by the American Mathematical Society through the intermediacy of the London Mathematical Society, has been installed in the Spencer Room, and is now available for use by Fellows. Lists of papers available in microfilm form will be issued from time to time.

Several requests for books on Astronomy have been received from prisoners of war; it was decided that such requests could best be met through the organization of the British Red Cross and a donation was made to assist in the purchase and dispatch of such books.

No binding has been done during 1943.

12. *The Society's Office and Staff.*—No changes have taken place during the year, though the Society has been fortunate in securing part-time secretarial assistance from Mrs. U. M. Dewey.

13. *The Society's Property and Instruments.*—The Society's more valuable property continues to be stored either in the strong room at Burlington House or at the University Observatory, Oxford.

Some essential repairs were carried out during the year, mainly to the seats in the meeting room; in addition, the lantern was completely overhauled and adjusted.

Various instruments have been lent and returned during the year. The list of instruments has again been omitted from this Report.

Collections of slides belonging to the late Rev. T. E. R. Phillips and the late Rev. G. Street have been gratefully accepted to supplement the collection of slides for hiring purposes to Fellows.

No new slides or celestial photographs have been added to the Society's collection during the year.

14. *The Observatory.*—In accordance with the agreement of 1941, Fellows have received the six 1943 issues of the magazine. The Editors have co-operated with the Council in the insertion of notices and information of particular interest to Fellows. Fellows are reminded that information about the Society's activities (for instance, details of papers available in microfilm form) will often be published solely in the pages of *The Observatory*.

The original agreement, which was for two years in the first instance, expired at the end of 1943; it has been renewed in a form which will operate until terminated,

with due notice, either by the Society or by the Editors. The block subscription paid to the Editors on behalf of Fellows has been increased from £100 to £110 per annum.

To save the heavy expense of separate distribution to Fellows, it has been agreed to circulate the magazine with *Monthly Notices* even though this may involve delay.

15. *The British Astronomical Association.*—During 1943 the British Astronomical Association continued to hold both Ordinary and Council meetings on the Society's premises. The Association's library is open to members from 13^h to 16^h on Wednesdays.

16. *The London Mathematical Society.*—Meetings of the London Mathematical Society were held on three occasions in the Society's rooms.

17. *Lectures for the Forces.*—In response to a demand for authoritative but popular lectures on astronomy, the Society, in collaboration with the British Astronomical Association, organized the following series of six lectures for members of the British and Allied Forces, to be given in the Society's rooms in Burlington House.

1943 Dec. 3. Sir James H. Jeans, O.M., F.R.S.
"Photographic Astronomy".

Dec. 17. Professor W. M. Smart, M.A., D.Sc.
"The Origin of the Earth".

1944 Jan. 7. B. M. Peek, Esq., M.A.
"The Planets and Planetary Observations".

Jan. 21. Professor H. Dingle, A.R.C.S., D.Sc.
"The Movements of the Stars".

Feb. 4. Will T. Hay, Esq.
"Life on Other Planets".

Feb. 18. F. J. Hargreaves, Esq., F.C.I.P.A. (President, British Astronomical Association)
"The Determination of Stellar Distances".

These lectures are being well attended by appreciative audiences. Short reports of the lectures are being printed in *The Observatory*.

LIST OF PUBLIC INSTITUTIONS AND OF PERSONS WHO HAVE PRESENTED GIFTS TO THE LIBRARY DURING THE YEAR 1943

Astrophysical Observatory of the Academy of Sciences of the Georgian S.S.R., Abastuman
Association of Scientific Workers
The British Council
Egyptian University, Cairo
Committee for the Continued Distribution of Astronomical Literature, Harvard College Observatory
The Editors of *The Observatory*
The Editors of *Sky and Telescope*
The Empire Press
Mathematical and Physical Society of Egypt
Gray Milne Trust
Griffith Observatory, Los Angeles
Imperial Chemical Industries
Institute of Actuaries
Institute of Physics
Repartição de Estatística, Lourenço Marques
National Bureau of Standards, Washington
Messrs. Thomas Nelson & Sons Ltd.
Academy of Sciences, New York
Observatorio de Fisica Cósmica, San Miguel
Society for Research on Meteorites, Los Angeles
H.M. Stationery Office
Astronomical Observatory, Tachkent
Tadjik Astronomical Observatory, Stalinabad
Messrs. C. A. Watts & Co. Ltd.
Mr. A. C. Banerji
Mr. F. C. Blythe
Mr. C. R. Bruce
Mr. M. Bunge
Dr. L. J. Comrie
Mr. R. Eisler
Sir J. C. W. Herschel
Sir Harold Spencer Jones
Mr. H. A. Lloyd
Dr. H. Macpherson
Dr. G. C. McVittie
Professor E. A. Milne
Mr. H. W. Newton
Mrs. Phillips
Mr. N. Sakellaropoulos
Dr. T. J. J. See
Mr. A. C. Serra
Dr. G. A. Shajn
Mrs. Street
Mr. E. Tillotson
Mr. F. Twyman
Rev. O. R. Walkey

PROGRESS AND PRESENT STATE OF THE SOCIETY

	Patron	Institutional Members	Fellows			Associates	Total
			Compounders	Annual Subscribers	Non-active Fellows		
1943 January 1	1	5	175	565	86	48	880
Since elected	...	...	...	+35	...	...	...
Since re-elected	...	...	...	+ 3	...	...	...
Election void †	...	...	...	− 1	...	...	...
Deceased, 1943	...	...	−5	− 8	...	−1	...
Deceased prior to 1943, death not notified	...	...	−1	− 4	...	...	...
Death presumed	...	...	−1	...	...	...	...
Resigned...	...	...	...	− 4	...	...	...
Name removed	...	...	...	...	− 1	...	...
Since compounded	...	...	+8	− 8	...	...	...
Suspense Account—							
Added 1943:							
(*a*) Subns. unobtainable under War conditions	...	...	...	− 7	+ 7	...	...
(*b*) Under Bye-law 26A... ...	...	...	...	− 5	+ 6	...	...
(*c*) Under Bye-law 26B... ...	...	...	...	− 7	+11	...	...
Transferred 1943:							
(*a*) From "War conditions" 1942 to Bye-law 26A ...	...	...	...	...	− 1	...	...
(*b*) Ditto to Bye-law 26B	...	...	...	...	− 4	...	...
(*c*) From Non-active 1942 to Active	...	...	+1	+ 3	− 4	...	...
1944 January 1	1	5	177	562	100	47	892*

(* As far as can be ascertained under present circumstances.)
(† Henry J. Green, elected 1943 March 12.)

(Nine Associates are also Fellows, and are therefore counted twice in the above table.)

Cash

RECEIVED

	£ s. d.	£ s. d.
Balances, 1943 January 1:—		
In hand of Assistant Secretary, on Petty Cash Account	10 9 1	
At Westminster Bank, on Current Account	644 16 8	
At Westminster Bank, on "B" Account:—		
A. S. Williams Bequest	52 10 0	
At Westminster Bank, on "C" Account:—		
Trust Funds £198 19 6		
Sales of Duplicate Books Fund 5 18 6		
Goodridge Bequest 8 11 8		
	213 9 8	
At Post Office Savings Bank:—		
General Fund £1,055 1 4		
"C" Account 505 4 2		
	1,560 5 6	
At London Savings Bank:—		
Ordinary Department (including £250 A. S. Williams Bequest) £1,047 6 6		
Special Investment Department 527 8 11		
	1,574 15 5	
		4,056 6 4
Proceeds East Indian Railway 3% New Debenture Stock	2,118 3 5	
Dividends on Investments	575 19 7	
Interest on Savings Banks Deposits	76 8 4	
Income Tax refunded: 1942 April–December £178 17 10		
1943 January–April 108 9 0		
	287 6 10	
Sales of Publications:—		
For 1943 £195 12 2		
Due in 1942 (including postages) 12 14 3		
Written off 1941 now paid 0 3 0		
1944 in advance 56 8 6		
	264 17 11	
Reprints:—		
For 1943 £7 10 2		
Due in 1942 5 4 7		
	12 14 9	
Sales of Photographs and Hire of Slides	4 16 11	
Miscellaneous Receipts:—		
London Mathematical Society £12 0 0		
Assistant Secretary, Pension Contribution 8 0 0		
Excess on Subscriptions 0 19 11		
Postages repaid 14 2 11		
Postages 1944, paid in advance 6 6 0		
British Astronomical Association, 1942 16 13 4		
British Astronomical Association, 1943 37 10 0		
British Astronomical Association, Headley Obs. Account, 1942 0 2 6		
	95 14 8	
		3,436 2 5
Received on Account of Subscriptions:—		
488 Annual Contributions, 1943 (one at 1 guinea re-elected)	1,023 15 0	
5 Institutional Membership Fees, 1943	10 10 0	
19 Annual Contributions, 1944, in advance	39 18 0	
½ ditto, 1945, in advance	1 1 0	
31 Admission Fees, 1943	65 2 0	
30 First Contributions, 1943	52 10 0	
Arrears paid off:—		
1 1923, 1924, 1925 £6 6 0		
1 1938 2 2 0		
2 1939 4 4 0		
5 1940 10 10 0		
10 1941 21 0 0		
49 1942 102 18 0		
1 Admission Fee and First Contribution, 1942 3 3 0		
	150 3 0	
		1,342 19 0
Received on account of application for Fellowship		4 4 0
Compositions:—		
2 Initial	63 0 0	
5 Reduced	56 14 0	
		119 14 0
"C" Account:—		
Proceeds of East Indian Railway 3% New Debenture Stock	542 18 2	
Proceeds of India 3½% Stock	1,122 19 6	
Dividends on Investments	147 15 0	
Income Tax refunded: 1942 April–December £35 7 0		
1943 January–April 5 14 7		
	41 1 7	
Interest on Post Office Savings Bank Account	13 17 6	
		1,868 11 9
Sales of Duplicate Books		20 14 0
Gift to Sales of Duplicate Books Fund		13 15 0
		£10,862 6 6

Account

PAID

	£ s. d.	£ s. d.
Assistant Secretary: Salary £287 10 9		
Inland Revenue: Income Tax on ditto 37 9 3		
	325 0 0	
Assistant Secretary: Pension Premium	24 0 0	
Health and Unemployment Insurance	3 18 0	
Work done for London Mathematical Society, 1942	5 0 0	
Clerical Assistance	63 4 0	
Porter and Wife: Wages and Cleaning £215 11 6		
Inland Revenue: Income Tax on ditto 4 6 0		
	219 17 6	
Porter and Wife: Pension Contribution	13 0 0	
Health and Unemployment Insurance	6 14 4	
		660 13 10
Insurance (general) and Telephone	35 2 6	
War Damage Insurance, paid up to 1944 March 31	70 0 0	
		105 2 6
Printing, etc.:—		
Monthly Notices (Vol. 102, No. 6; Vol. 103, Nos. 1–4)	535 12 0	
Geophysical Supplement (Vol. 5, No. 3)	76 3 4	
Reprints	32 3 4	
Miscellaneous	19 16 1	
Photo-engraving	20 15 11	
		684 10 8
Postage and Packing:—		
Monthly Notices, etc.	43 14 2	
Office Postages...	45 3 7	
		88 17 9
A.R.P. Expenditure	72 15 6	
Stationery and Office Expenses	57 4 10	
Heating and Lighting	94 13 8	
Accountant's Fee	31 10 0	
Subscription to Committee on Library Co-operation	1 1 0	
Donation to British Red Cross Society (Astronomical Books for Prisoners of War)	10 0 0	
Authorized Travelling Expenses, 1942 ... ... £0 12 0		
" " " 1943 ... ... 4 11 3		
	5 3 3	
Repairs and Cleaning of Fittings and Fixtures £67 17 11		
House Expenses and Laundry 17 8 6		
Expenses of R.A.S. Meetings and Committees 16 16 2		
Miscellaneous Expenditure... ... ... 8 13 2		
Bank Charges and Cheque Books... ... ... 0 19 0		
Library Expenditure 0 14 2		
	112 8 11	
Gold Medal	13 14 1	
		398 11 3
Purchase of Books from Potter Fund		17 0 3
Headley Observatory:—		
Rent of Observatory Paddock £5 0 0		
Insurance of Buildings and Instruments on Site 2 11 6		
Mr. Maskell for care of Site 2 12 0		
Hire of Electric Kettle 0 4 6		
	10 8 0	
Less Contribution from British Astronomical Association £2 12 6		
Contribution from Mrs. Phillips 0 7 6		
	3 0 0	
The Observatory :—.		7 8 0
Block Subscription	100 0 0	
Grant towards printing costs, Newton Addresses	15 0 0	
Grant to cover cost of distribution, 1942	35 0 0	
		150 0 0
Lectures for the Forces:—		
Printing	4 8 0	
Lecturers' Travelling Expenses	5 19 1	
Porters' Attendance	1 10 0	
		11 17 1
Purchase of 3% Savings Bonds, 1960–1970		2,118 3 5
Expenditure on Trust Accounts:—		
Purchase of 3% Savings Bonds, 1960–1970	1,665 17 8	
Turnor and Horrocks Fund (Library)	14 1 9	
Harry Watson Fund (Library)	1 18 9	
Lee and Janson Fund...	45 0 0	
George Darwin Lectureship Fund	40 0 0	
		1,766 18 2
Purchase of Books from Sales of Duplicate Books Fund		86 6 10
Balances, 1943 December 31:—		
In hand of Assistant Secretary, on Petty Cash Account	8 9 6	
At Westminster Bank, on Current Account, as per Pass Book £748 10 8		
Less Cheques not yet presented... ... ... 17 8 1		
	731 2 7	
At Westminster Bank, on "B" Account (A. S. Williams Bequest)	52 10 0	
At Westminster Bank, on "C" Account:—		
Trust Funds £114 10 10		
Sales of Duplicate Books Fund... ... ... 4 11 1		
Goodridge Bequest 10 6 0		
	129 7 11	
At Post Office Savings Bank:—		
General Fund £1,590 16 4		
"C" Account 639 1 8		
	2,229 18 0	
At London Savings Bank:—		
Ordinary Department (including £250 A. S. Williams Bequest) £1,073 10 0		
Special Investment Department 541 18 9		
	1,615 8 9	
		4,766 16 9
		£10,862 6 6

Dr. *General Fund Revenue Account*

	£	s.	d.	£	s.	d.
To Salaries and Wages, including Pension Premiums and National Insurance				584	9	10
,, Clerical Assistance				63	4	0
,, Insurance and Telephone				35	2	6
,, War Damage Act, 1941, Part II: Premiums to 1943 December 31				87	10	0
,, Printing, etc. (after adjusting Reserves)				1,009	10	8
,, Posting and Packing (after adjusting Reserves)				74	14	10
,, *General Expenses (after adjusting Reserves):—*						
Stationery and Office Expenses	57	4	10			
Lighting and Heating	91	0	8			
Travelling Expenses	5	3	3			
Subscription to Committee on Library Co-operation	1	1	0			
Accountants' Fee	31	10	0			
A.R.P. and Fire Watchers' Expenses	74	5	6			
Donation to British Red Cross	10	0	0			
Gold Medal	13	14	1			
Repairs and Cleaning of Fittings and Fixtures	67	17	11			
Sundries	47	1	0			
				398	18	3
,, *The Observatory:—*						
Block Subscription	100	0	0			
Grant towards printing costs—Newton addresses	15	0	0			
Grant to cover costs of distribution, 1942	35	0	0			
				150	0	0
,, Purchase of Books from Potter Fund				17	0	3
,, Headley Observatory, Expenses *less* Refunds				7	8	0
,, Lectures for the Forces, Expenses *less* Refunds				5	18	7
,, Annual Reserve for Repairs				40	0	0
,, Staff Pension Fund: Amount set aside for year				50	0	0
,, Net Revenue one year to date transferred to Balance Sheet				84	3	6
				£2,608	0	5

One Year to 1943 December 31 *Cr.*

	£ s. d.	£ s. d.	£ s. d.
By *Amounts received from Fellows, viz.:—*			
1 Arrear for 1923, 1924, 1925	6 6 0		
1 „ „ 1938	2 2 0		
2 Arrears for 1939	4 4 0		
5 „ „ 1940	10 10 0		
10 „ „ 1941	21 0 0		
49 „ „ 1942	102 18 0		
1 Arrear, Admission Fee and First Contribution, 1942	3 3 0		
488 Annual Contributions for 1943 (including 8 from Fellows deceased 1943)	1,023 15 0		
5 Institutional Membership Fees	10 10 0		
15 Prepayments for 1943	31 10 0		
		1,215 18 0	
31 Admission Fees	... ...	65 2 0	
30 First Contributions	... ...	52 10 0	
Composition Fees:—			
Initial Fees	63 0 0		
Reduced Fees...	56 14 0		
	119 14 0		
Less Transferred to Reserve Account ...	119 14 0		
	– – –		
Add Amount brought to Credit for the year	52 12 2		
		52 12 2	
			1,386 2 2
„ Interest and Dividends received (gross)		845 4 11	
„ Interest on Bank Deposit Accounts received		76 8 4	
			921 13 3
„ Sales of Publications, Photographs and Miscellaneous Receipts for the year			300 5 0
			£2,608 0 5

Note.—Fellows' 1943 Contributions not received were:—

63 Unobtainable under War conditions	£132 6 0
26 Waived under Bye-law 26A	54 12 0
11 Waived under Bye-law 26B	23 2 0
37 Others unpaid at 1943 December 31	77 14 0
3 Admission Fees and First Contributions, ditto	10 10 0
Total for the year	£298 4 0

This amount has been added to the Suspense Account and is not included in this Revenue Account.

Balance Sheet

LIABILITIES

	£ s. d.	£ s. d.
1 Contribution for 1944, paid in 1942	2 2 0	
19 Contributions for 1944, paid in 1943	39 18 0	
½ Contribution for 1945, paid in 1943	1 1 0	
Received on application for Fellowship	4 4 0	
		47 5 0
Sundry Creditors for Accounts due but not presented		470 10 0
Reserve for Repairs and Maintenance:—		
As at 1942 December 31	80 0 0	
Add Set aside 1943	40 0 0	
		120 0 0
Trust Funds and Goodridge Bequest:—		
Capital as at 1942 December 31	4,582 2 7	
Add Excess over Book Value realised on Transfer of Stock to Treasury	674 17 8	
	5,257 0 3	
Income Balances at 1943 December 31	763 18 6	
Income Tax on Trust Funds, 1943 April–December, not yet refunded	29 8 3	
		6,050 7 0
The Grove-Hills Fund (Capital as per contra)		1,480 2 2
Sales of Duplicate Books Fund:—		
In hand at 1942 December 31	56 8 11	
Add Received for Duplicate Books sold	20 14 0	
Received by Gift to Fund	13 15 0	
	90 17 11	
Less Books, purchased, 1943	86 6 10	
		4 11 1
Composition Fees Reserve Fund:—		
As at 1942 December 31	757 2 6	
Received in 1943	119 14 0	
	876 16 6	
Less 6% transferred to Current Account	52 12 2	
		824 4 4
Franklin-Adams Charts:—		
As at 1942 December 31		170 7 8
Advance Receipt for Publications, 1944	56 8 6	
Advance Receipt for Postages, 1944	6 6 0	
		62 14 6
Credits still in hand from 1939		0 12 3
Staff Pension Fund:—		
As at 1942 December 31	50 0 0	
Add Amount set aside in 1943	50 0 0	
		100 0 0
General Fund:—		
As at 1942 December 31	20,284 16 5	
Add Excess over Book Value realised on Transfer of Stock to Treasury	894 3 5	
	21,178 19 10	
Add Excess of Revenue over Expenditure, 1943	84 3 6	
		21,263 3 4
		£30,593 17 4

To the Fellows of THE ROYAL ASTRONOMICAL SOCIETY.

We have examined the above Balance Sheet with the Books and Vouchers relating thereto explanations given to us.

We have verified the Securities representing the Investments and have found them to be

1943 *December* 31

ASSETS

	£ s. d.	£ s. d.
Sundry Debtors, 1943		37 14 8
War Damage Act, Part II, Premiums paid in advance		17 10 0
Investments of General Fund, valued as at 1922 December 29 or subsequent cost		18,844 8 11
Investments of the Grove-Hills Fund, valued at cost		1,480 2 2
Investments of Trust Funds and Goodridge Bequest, valued at cost...		5,257 0 3
Income Tax, 1943 April–December, deducted at source, not yet refunded:—		
General Fund	160 16 4	
Trust Funds	29 8 3	
		190 4 7
Sundry Balances, as per Cash Account		4,766 16 9

Note.—Fellows' Contributions and 3 *Admission Fees unpaid at* 1943 *December* 31 *:* £808, 10*s.* 0*d. This amount represents the total of the Suspense Account and is not included in this Balance Sheet.*

£30,593 17 4

and certify it to be correctly drawn up therefrom and in accordance with the information and in order.

SUFFOLK HOUSE,
5 LAURENCE POUNTNEY HILL, LONDON, E.C. 4,
1944 *April* 4.

SHARP, PARSONS & CO.,
Chartered Accountants.

INVESTMENTS

As at 1943 December 31

General Fund

£2000 Swansea Corporation 3½ per cent. Stock.
£5125 London, Midland and Scottish Railway 4 per cent. Debenture Stock.*
£750 London, Midland and Scottish Railway 4 per cent. Preference Stock.*
£2400 Gas Light and Coke Co. 3 per cent. Consolidated Debenture Stock.*
£2000 Commercial Gas Co. 3 per cent. Debenture Stock.*
£496 Consolidated 4 per cent. Stock, 1957.
£500 Agricultural Mortgage Corporation, Ltd., 4½ per cent. Debenture Stock, 1961–91.*
£1000 Hull Corporation 4½ per cent. Redeemable Stock, 1952–72.
£1000 Central Electricity Board 4½ per cent. Stock, 1957–82.*
£700 Birmingham Corporation 3 per cent. Inscribed Stock, 1947.
£1807, 9*s.* 0*d.* War Loan 3½ per cent. Inscribed Stock.
£500 New South Wales 4 per cent. Stock, 1942–62.*
£1156, 1*s.* 5*d.* Metropolitan Water Board 3 per cent. "B" Stock.*
£500 National Defence Loan, 1954–58, 3 per cent. Bonds.*
£500 National War Bonds 2½ per cent., 1946–48.*
£2118, 3*s.* 5*d.* Savings Bonds 3 per cent., 1960/1970.* (A Holding.)

Grove-Hills Fund

£423, 15*s.* 11*d.* War Loan 3½ per cent. Inscribed Stock.
£695, 16*s.* 0*d.* Conversion Loan 3½ per cent., 1961.
£535 Agricultural Mortgage Corporation, Ltd., 4½ per cent. Debenture Stock, 1961–91.*

Trust Funds

£1004 Consolidated 4 per cent. Stock, 1957.
£965 Agricultural Mortgage Corporation, Ltd., 4½ per cent. Debenture Stock, 1961–91.*
£540, 10*s.* 4*d.* War Loan 3½ per cent. Inscribed Stock.
£1160, 16*s.* 3*d.* War Loan 3½ per cent. Inscribed Stock.
£542, 18*s.* 2*d.* Savings Bonds 3 per cent., 1960/1970.* (B Holding.)
£1122, 19*s.* 6*d.* Savings Bonds 3 per cent., 1960/1970.* (C Holding.)

* *Taxed at source.*

GIFTS TO THE GENERAL FUNDS AND PROPERTY OF THE SOCIETY

The John Lee Gift: In 1836 John Lee, Esq., LL.D., gave the Advowson of the living of Hartwell; in 1844 he gave the Advowson of the living of Stone, Bucks. In 1879 these Advowsons were repurchased by his heir, Edward Lee, Esq., for £700.

The Lawson Bequest (1856): Henry Lawson, Esq., bequeathed to the Society the sum of £200, free of legacy duty.

The Carrington Bequest (1876): R. C. Carrington, Esq., F.R.S., bequeathed to the Society £2000 Consols.

The Lambert Bequest (1877): C. J. Lambert, Esq., presented to the Society the sum of £500, free of legacy duty, being part of a sum of money bequeathed by his father, Charles Lambert, Esq., for scientific purposes.

The McClean Bequest (1905): Frank McClean, Esq., LL.D., F.R.S., bequeathed to the Society the sum of £2000, free of legacy duty.

The Farrar Bequest (1906): The Rev. A. S. Farrar, D.D., bequeathed to the Society the sum of £100, free of legacy duty.

The Parsons Gift (1922): The Hon. Sir Charles Parsons, K.C.B., F.R.S., gave to the Society £2500 War Stock 1929–47, in memory of his father, the 3rd Earl of Rosse, who made the 6-foot reflector, and who died 1867 October 31.

The Grove-Hills Bequest (1922): Colonel E. H. Grove-Hills, C.M.G., F.R.S., bequeathed to the Society, free of legacy duty, his collection of early astronomical and mathematical books up to the year 1700, together with a sum of £250 to cover the expenses incurred by the Society in connection with the books.

The Lindemann Bequest (1931): A. F. Lindemann, Esq., bequeathed to the Society the sum of £1000, free of legacy duty.

The Archdeacon Potter Bequest (1933) (*see below*).

The Herbert Spencer Bequest (1936): Herbert Spencer, Esq., who died in 1903, nominated the Royal Astronomical Society as one of twelve Learned Societies to be eventual beneficiaries of his residuary estate. His estate was wound up in 1936, and the Society's share of the residue amounted to £1241. The conditions of acceptance of the legacy were that the sum received should be spent in certain specified ways by the Governing Body within five years of the date of payment, and might not be used in any way for purposes of endowment. The bequest was finally expended during 1941.

The Goodridge Bequest (1937) (*see below*).

The Lindley Bequest (1937): Miss Julia Lindley bequeathed to the Society the sum of £1000, free of legacy duty, which has been received from the Public Trustee in the form of £1156, 1*s.* 5*d.* Metropolitan Water Board 3 per cent. "B" Stock.

The Stanley Williams Bequest (1939): A. Stanley Williams, Esq., who died 1938 November 21, bequeathed to the Society all his astronomical instruments, books, maps, photographs, manuscripts, etc., together with the sum of £200, free of legacy duty, for the reduction or publication of his observations of variable stars; and also a further sum of £100, free of legacy duty, for the furtherance of astronomy in general or of the Society in particular, in such manner as the Council of the Society may think fit. No expenditure from the fund has been made up to 1943 December 31.

SPECIAL FUNDS

The Grove-Hills Fund (1922): In 1922 the Treasurer, Colonel Grove-Hills, collected by his private efforts, with the approval of the Council, a Special Fund to meet the immediate financial difficulties of the Society. The total amount received from all sources (including dividends on the investments representing the fund) has been £1913, 1*s.* 11*d.*, of which £1480, 2*s.* 10*d.* has been invested and is now represented by £535 Agricultural Mortgage 4½ per cent., £423, 15*s.* 11*d.* War Stock 3½ per cent., and £695, 16*s.* Conversion Loan 3½ per cent. 1961.

The Archdeacon Potter Fund (1933): The late Archdeacon Beresford Potter, who died 1931 May 10, bequeathed to the Society a 3/88th share of his residuary estate. This legacy was received in 1933, and now stands invested in £300 3½ per cent. War Stock, the cash balance of £17, 15*s.* 2*d.* being expended on the library in 1934. The Council resolved to allocate the dividends from this fund for the purposes of the library.

	£	*s.*	*d.*		£	*s.*	*d.*
1943 Jan. 1. Balance of Fund ...	62	14	0	1943. Purchase of Books ...	17	0	3
1943. Dividends, War Stock 3½ per cent.	10	10	0	1943 Dec. 31. Balance of Fund	56	3	9
	£73	4	0		£73	4	0

The Goodridge Bequest (1937) *: Captain J. J. L. Goodridge, who died 1936 March 13, bequeathed to the Society the sum of £50, free of legacy duty. In 1937 the bequest was invested in £49, 0*s.* 4*d.* War Stock 3½ per cent.

	£	*s.*	*d.*		£	*s.*	*d.*
1943 Jan. 1. Balance of Fund ...	8	11	8	1943 Dec. 31. Balance of Fund	10	6	0
1943. Dividends, War Stock 3½ per cent.	1	14	4				
	£10	6	0		£10	6	0

TRUST FUNDS

The Lee and Janson Fund (1834 and 1879): In 1834 John Lee, Esq., LL.D., gave £100, and in 1879 T. C. Janson, Esq., bequeathed £200, the interest to be given to the widow or orphan of a deceased Fellow. These amounts now stand in £309, 16*s.* 3 per cent. Savings Bonds, 1960/1970.

* This bequest is invested with the Stillhamer Trust, making up the total of £540, 10*s.* 4*d.* 3½ per cent. War Loan.

	£	s.	d.		£	s.	d.
1943 Jan. 1. Balance of Fund ...	73	7	6	1943 Jan. 1. Grant from Fund ...	45	0	0
1943. Dividends, East Indian Railway	1	5	1	1943 Mar. 19. Purchase of £309, 16*s.* 3 per cent. Savings Bonds, 1960/1970	309	16	0
1943 Mar. 18. Proceeds of £334, 10*s.* 9*d.* East Indian Railway 3 per cent. Debenture Stock acquired by the Treasury	309	16	0	1943 Dec. 31. Balance of Fund:— In P.O. Savings Bank £34 14 11			
1943. Dividends, 3 per cent. Savings Bonds	2	2	6	At Bank 2 15 0			
Income Tax, 1942 April–1943 April, refunded	5	0	4		37	9	11
Interest on P.O. Savings Bank Deposit	0	14	6				
	£392	5	11		£392	5	11

The Turnor Fund and the Horrocks Memorial Fund (1853 and 1876): In 1853 The Rev. Charles Turnor, M.A., bequeathed a sum of £500, which now stands in £430, 10*s.* 4*d.* 3 per cent. Savings Bonds, 1960/1970. In 1876 the Society received a sum of £38, 15*s.*, the unexpended balance of a fund collected for the purpose of erecting a memorial in Westminster Abbey to Jeremiah Horrocks: to this the Society added the sum of £55, 15*s.*, and the whole now stands in £95, 13*s.* 2*d.* 3 per cent. Savings Bonds, 1960/1970. The interest on these funds is to be used in the purchase of books for the library.

	£	s.	d.		£	s.	d.
1943 Jan. 1. Balance of Fund ...	4	0	6	1943 Mar. 19. Purchase of £526, 3*s.* 6*d.* 3 per cent. Savings Bonds, 1960/1970 ...	526	3	6
1943. Dividends, East Indian Railway	2	2	8	1943. Purchase of Books ...	14	1	9
1943 Mar. 18. Proceeds £560, 11*s.* 2*d.* East Indian Railway Stock acquired by the Treasury	526	3	6	1943 Dec. 31. Balance of Fund...	4	4	2
1943. Dividends, 3 per cent. Savings Bonds	3	12	3				
Income Tax, 1942 April–1943 April, refunded	8	10	6				
	£544	9	5		£544	9	5

The Hannah Jackson (née Gwilt) Fund (1861): In 1861 Mrs. Hannah Jackson (*née* Gwilt) gave £300 Consols, which now stands in £287 3 per cent. Savings Bonds, 1960/1970; the interest to be given in medals or other awards in accordance with the terms of the Trust.

	£	s.	d.		£	s.	d.
1943 Jan. 1. Balance of Fund ...	19	18	2	1943 Mar. 19. Purchase of £287 3 per cent. Savings Bonds, 1960/1970	287	0	0
1943. Dividends, East Indian Railway	1	3	3	1943 Dec. 31. Balance of Fund...	27	13	10
1943 Mar. 18. Proceeds of £309, 18*s.* 6*d.* East Indian Railway 3 per cent. Debenture Stock acquired by the Treasury	287	0	0				
1943. Dividends, 3 per cent. Savings Bonds	1	19	5				
Income Tax, 1942 April–1943 April, refunded	4	13	0				
	£314	13	10		£314	13	10

The Harry Watson Memorial Fund (1923): In 1920 Mrs. Watson presented to the Society the sum of £300 as a memorial to her late husband, Lieut.-Col. Harry James Watson, the interest on this sum to be used for the purchase of books for the library, such books to be indicated by a special book-plate. This gift now stands invested in £542, 18*s.* 2*d.* 3 per cent. Savings Bonds, 1960/1970. In 1923 Mrs. Watson bequeathed a further sum of £958, 17*s.* to the Society, which now stands invested in £965 Agricultural Mortgage 4½ per cent. Stock. The Council resolved that these two sums should together constitute a Trust Fund, to be designated *The Harry Watson Memorial Fund*, to be used for the purchase and binding of books.

	£	s.	d.		£	s.	d.
1943 Jan. 1. Balance of Fund ...	184	17	0	1943 Jan. 27. Purchase of £542, 18*s.* 2*d.* 3 per cent. Savings Bonds, 1960/1970 ...	542	18	2
1943. Dividends, India 3½ per cent.	4	15	0	1943. Purchase of Books ...	1	18	9
1943 Jan. 27. Prooceeds of £542, 18*s.* 2*d.* India 3½ per cent. Stock acquired by the Treasury	542	18	2	1943 Dec. 31. Balance of Fund:— In P.O. Savings Bank £222 10 3			
1943. Dividends, Agricultural Mortgage 4½ per cent. ...	21	14	4	At Bank 24 0 7			
Dividends, 3 per cent. Savings Bonds	9	6	6		246	10	10
Income Tax, 1942 April–1943 April, refunded	22	17	9				
Interest on P.O. Savings Bank Deposit	4	19	0				
	£791	7	9		£791	7	9

The George Darwin Lectureship Fund (1926): In 1926 Sir James Jeans presented to the Society the sum of £1000 for the endowment of an annual lectureship, to be called *The George Darwin Lectureship*, the lecture to be on some subject of interest to astronomers, preference being given, in electing a lecturer, to one normally resident outside the British Isles.

This sum now stands in £1004 Consolidated 4 per cent. Loan. The income is annually applied to the purposes of the Trust in accordance with the terms of the Deed of Gift.

	£	s.	d.		£	s.	d.
1943 Jan. 1. Balance of Fund ...	141	15	1	1943 Sept. Lecture Fee	40	0	0
1943. Dividends, Consolidated 4 per cent. Loan	40	3	2	1943 Dec. 31. Balance of Fund:— In P.O. Savings Bank £123 11 1			
Interest on P.O. Savings Bank Deposit	2	10	3	At Bank 20 17 5			
					144	8	6
	£184	8	6		£184	8	6

The A. G. Stillhamer Trust (1937): The late A. G. Stillhamer, Esq., of Bloomington, Illinois, left all his property outright to Miss Maude S. Capps, of Bloomington; but on account of certain personal memoranda and conversation between Mr. Stillhamer and Miss Capps, the latter resolved to carry out what she believed to have been Mr. Stillhamer's wishes by the payment to the Royal Astronomical Society of the sum of 2500 dollars in cash, the conditions of acceptance being that the principal is to remain intact in the endowment fund of the Society, and the income therefrom to be used annually * to pay expenses of scientific research or to purchase definite

* Miss Capps, subsequently to the initial conditions of the Trust, agreed to the income not being expended annually.

scientific instruments for use in scientific instruction or research. The bequest has been invested in £491, 10*s*. War Stock 3½ per cent.

	£	*s.*	*d.*		£	*s.*	*d.*
1943 Jan. 1. Balance of Fund ...	86	10	5	1943 Dec. 31. Balance of Fund:—			
1943. Dividends, War Stock 3½ per cent.	17	4	0	In P.O. Savings Bank £93 2 2			
Interest on P.O. Savings Bank Deposit	2	1	9	At Bank 12 14 0			
					105	16	2
	£105	16	2		£105	16	2

The E. W. Brown Trust (1939): E. W. Brown, Esq., M.A., Sc.D., F.R.S., who died in 1938, bequeathed to the Society the sum of five thousand dollars, and desired that the income from the bequest should be used for assistance in the calculation of accurate orbits of the bodies of the solar system, such income as should not be used for this purpose being used for the general purposes of the Society. The bequest was received in 1939 July, and was invested in £1160, 16*s*. 3*d*. War Stock 3½ per cent.

	£	*s.*	*d.*		£	*s.*	*d*
1943 Jan. 1. Balance of Fund ...	143	4	7	1943 Dec. 31. Balance of Fund:—			
1943. Dividends, War Stock 3½ per cent.	40	12	6	In P.O. Savings Bank £164 12 10			
Interest on P.O. Savings Bank Deposit	3	12	0	At Bank 22 16 3			
					187	9	1
	£187	9	1		£187	9	1

REPORT OF THE AUDITORS FOR THE YEAR 1943

We have examined the accounts of the Royal Astronomical Society and we approve of the various items of expenditure shown therein. We have checked the official list of Fellows against subscriptions received, together with the other figures, so far as they were available, which were used by the professional auditors. It is gratifying to note that about £150 has been received from Fellows for arrears of payment. The instruments on the Society's premises have been examined and appear to be satisfactory, but it is understood that no check has been made of the volumes in the Library for some considerable time.

In the course of our inquiries we have received every assistance from the Assistant Secretary and, with regard to checking the list of instruments, from the porter.

C. C. L. GREGORY.
E. G. MARTIN.

OBITUARY NOTICES

ROBERT MORRISON ABRAHAM was born in Coleraine, Northern Ireland, on 1879 March 8, and died at Tewin, Herts, on 1943 July 26. He was educated in Coleraine and at King's College, London, and was a member of the Institute of Electrical Engineers.

He served with Siemens of Woolwich, and later with firms in Belfast, in South Africa, Australia, and America, and finally he joined Casella's in London, of which firm he became a director in 1911. In 1926 he published a book on the design, construction and testing of surveying instruments.

He married Mary Price Owen in 1908, and is survived by a widow and three sons.

He was elected a Fellow of the Society on 1925 November 3.

ARTHUR HENRY BAGNOLD was born on 1854 March 18 and died on 1943 December 9. He was the son of Major-General M. E. Bagnold, East India Company, and was educated at Cheltenham and at Woolwich, where he was contemporary with Kitchener and the Prince Imperial. He passed into the Royal Engineers in 1872, serving in South Africa in 1881, and in the Sudan in 1884 and 1885 with distinction. He reached the rank of Colonel in 1903 and was made a C.M.G. for his services in the 1914–1918 war.

In the course of his career he had been an instructor at the School of Military Engineering from 1891 to 1896, and superintendent of building works at Woolwich Arsenal from 1903 to 1911, when he retired. He had been made a C.B. in 1907. When the last war broke out he returned to active service and was appointed military assistant to the Chief Superintendent of Ordnance Factories, a post he held until 1916. Next he served as chief experimental officer, Signals Experimental Establishment, until 1917. In this connection it is interesting to note that somewhere about his twentieth birthday, when a subaltern at Aldershot, he put up with his own hands, experimentally, the first pair of Bell telephone receivers, brought over to England by Sir William Preece.

In 1888 he married Ethel, daughter of Mr. W. H. Alger. She died in 1931. Their son is Colonel R. A. Bagnold, the explorer, and their daughter, the wife of Sir Roderick Jones, is well known as a writer under the name of Enid Bagnold.

He was elected a Fellow of the Society on 1904 December 9.

SIDNEY GERALD BURRARD was the eldest son of Lieutenant-Colonel Sidney Burrard of the Grenadier Guards. He was born at the Mount, Yarmouth, Isle of Wight, on 1860 August 12. His father was the third son of the Rev. Sir George Burrard, 3rd Baronet of Walhampton in Hampshire. Late in life Sidney Gerald Burrard inherited the baronetcy from a cousin, becoming 7th Baronet. He entered the Royal Engineers in 1879 and went to India in 1882. In 1884 he was given an appointment in the Survey of India and was posted to the Trigonometrical Branch, in which he was to carry out such valuable geodetic researches.

In this brief memoir I shall confine myself to those parts of his career which were of an astronomical character. I may mention here that he was elected a Fellow of the Society on 1885 April 10. The first geodetic operations in which he took

part were measurements of arcs of longitude, using transit telescopes for the determination of local time and the telegraph wires for the exchange of time signals. This work had been going on intermittently for several years and the results were found to be liable to very puzzling circuit errors. If the three arcs of longitude, A to B, B to C and A to C were measured, clearly the sum of the first two should be equal to the third; but it was found that there were discrepancies much larger than the apparent probable errors of the measurements could account for. Every effort to discover the cause of these discrepancies had failed. The only suspicious features were changes in the collimation when the telescopes were reversed. It was thought that the telescopes were insufficiently rigid and they were sent to the makers, Messrs. Cooke & Sons of York, for examination. No fault was found. The Astronomer Royal, Sir George Airy, interested himself in the problem, and the telescopes were set up at the Royal Observatory and tested with care; still no fault could be found, and they went back to India. In the winter of 1889–90, when Burrard and the writer of this memoir were making longitude observations, Burrard discovered the cause. It was not that the telescopes were faulty but that the method of determining the collimation was to blame; this was the two-collimator method which, if there is any fault in the object-glasses of the collimators or in the adjustment of their focus, is liable to give erroneous results. The step to take was clear, namely to use the same value of the collimation for both positions of the telescope and balance the programme of observations. Fortunately this balance had always been kept, and when the old arcs were recomputed by the new method all the old circuit errors disappeared. The doubts as to the accuracy of these measurements having been cleared away, it was decided to make a new determination of the longitude of India starting from the Prime Meridian. It was decided to break the total arc into four parts, namely Greenwich-Potsdam, Potsdam-Teheran, Teheran-Bushire and Bushire-Karachi. Burrard was in command of the operations and the present writer was his colleague. For the work a new pair of transit instruments with convenient means of reversal was obtained. There is nothing of special interest to record. There were many delays due to cloudy skies and sometimes to telegraphic interruptions, and the work took eighteen months. It was finished in 1896 May.

The next event of astronomical interest was in 1898 January, when a total eclipse of the Sun took place. The central line of the shadow passed right across India, and in order to smooth the path for astronomers coming from England the Government of India gave sanction for the preparation of two camps at places on the central line, not too near each other, so as to reduce the risk of interference by passing clouds, and situated conveniently near to railway stations. The establishment of one of these camps was entrusted to Burrard. The place chosen by him was Sahdol in the Central Provinces. The principal guests were Sir William Christie (Astronomer Royal) and Professor H. H. Turner. The camp was a great success, the weather perfect and the observations good. As one of the objects in view was the determination of the position of the Moon, for which the longitude of the place of observation had to be known, it was fortunate that the value obtained two years before was available.

Burrard's greatest contribution to knowledge was made in 1901 when he published the important paper called "The Attraction of the Himalaya Mountains upon the Plumb-line in India". It is No. 5 in the series of Professional Papers of the Survey of India. To attempt to explain what this paper did for geodetic research would take more space than is available, and as the work is not of astronomical interest it would not be particularly appropriate in this memoir. It will be sufficient to say that it showed that the study of the deflection of the plumb-line would throw light on many problems of the structure of the Earth's crust. The importance of the study of deflections, that is to say of the direction of gravity, led very naturally to a desire for the investigation of the force of gravity, so it was decided to obtain a modern pendulum apparatus. The visit to Potsdam in 1895 in connection with the longitude operations had led to very

friendly relations with the Prussian Geodetic Institute which is situated in Potsdam, and the Director, Professor Helmert, very kindly consented to receive the apparatus from the makers, to test it and to determine the constants of the pendulums. Burrard and I were both in England in 1902 and we went together to Potsdam to study the technique of the observations, and we afterwards shared the work of determining the periods of the pendulums at Kew Observatory. Very shortly after this Burrard became Superintendent of the Trigonometrical Survey and so these observations were the last that he had occasion to make.

In 1904 he was elected a Fellow of the Royal Society. During the remainder of his time in India he was principally engaged on administrative duties, but his interest in scientific matters never flagged and he published a number of papers. In 1901, in collaboration with Mr. H. (afterwards Sir Henry) Hayden of the Geological Survey of India, he published a valuable book called *A Sketch of the Geography and Geology of the Himalaya Mountains and Tibet*, and in 1912 a paper entitled " The Origin of the Himalaya Mountains and Gangetic Trough ". He became Surveyor General of India in 1911 and in the same year was decorated with the C.S.I.; in 1914 he became K.C.S.I. In the ordinary course his tenure of the office of Surveyor General would have ended in 1915, but the Government of India very wisely extended his tenure for the duration of the war, so that he did not retire from the service till 1919. This extended tenure ensured that the Survey Department should have the benefit of his wise and kindly guidance all through that difficult period. When he left India he carried with him the gratitude of the Government and the affection and admiration of those who had served under him.

Mention must be made of one interesting event that took place during the war. A move had been made shortly before the war for the creation in India of a body resembling the British Association, and in 1914 the Asiatic Society of Bengal undertook to organize an annual congress after the pattern of the meetings of the British Association. The first took place in 1914 in Calcutta, the second in 1915 in Madras and the third in 1916 in Lucknow, on this last occasion Sir James (afterwards Lord) Meston was the Patron and Sir Sidney Burrard the President. In his address he dealt with the problem of mountain formation and elaborated the theme that he had written upon in 1912. This address was printed in full in the *Proceedings* of the Asiatic Society and is also to be found as an Appendix to Volume IX of *The Records of the Survey of India*.

After leaving India Sir Sidney settled in Farnborough and lived there until his death in 1943.

In 1887 he married Miss Gertrude Ellen Haig, elder daughter of Major-General C. T. Haig, R.E. There were two children of this marriage, a son Gerald, who is now 8th Baronet, and a daughter. Lady Burrard died in 1928. During the last years of her life she had suffered very much from arthritis and this had made her to a great extent a prisoner in her house.

In 1935 Sir Sidney married Miss Alice Simons, but three years later was again left a widower. The last years of his life were years of sadness, for his eyesight had deteriorated so that he was unable to read.

He was a man of wide accomplishment—a tolerable musician and an admirable painter in water colour. His interests were many and he could speak on almost any subject in an interesting way. As a companion and colleague he was incomparable.

G. P. LENOX-CONYNGHAM.

CUMBERLAND CLARK was elected a Fellow of the Society on 1921 February 11. His interests were varied and he was also a Fellow of the Royal Geographical Society and the Zoological Society.

He died in 1941 April as a result of enemy action.

LEONARD DARWIN was the fourth and last surviving son of Charles Darwin. He was born in 1850, educated at the Royal Military College, Woolwich, and commissioned in the Royal Engineers in 1871. During his military service he was a member of the expeditions to observe the Transits of Venus in 1874 and 1882. In 1874 he observed at a station in New Zealand, but in 1882, when he was one of the official observers at Brisbane, the observations were made impossible by dense cloud and rain. However, at that time the longitudes of all the Australian stations were doubtful, so Darwin returned *via* Singapore and completed the chain by making the necessary interchange of telegraphic signals with Port Darwin.

In 1886 he and Arthur Schuster observed the total solar eclipse at Prickly Point, Grenada, B.W.I., photographing the corona and the solar prominences with a prismatic camera.

Retiring from the army in 1890, Major Darwin took an active part in politics, and sat as Liberal Unionist M.P. for the Lichfield Division of Staffordshire from 1892 to 1895. He was President of the Royal Geographical Society from 1908 to 1911, was Chairman of Bedford College for Women, London University, from 1913 to 1920, and throughout the 1914–1918 war was Chairman of the Professional Classes Relief Council.

Latterly he concentrated his attention on Eugenics, and was President of the Eugenic Education Society. In 1926 he published a book entitled *The Need for Eugenic Reform* and that volume will probably for long be accepted as the standard presentation of the case for eugenics.

His first wife, Elizabeth, daughter of the late Mr. G. R. Fraser, died in 1898. In 1900 he married, secondly, Charlotte Mildred, daughter of the late Mr. E. Langton. She died in 1940.

Major Darwin was elected a Fellow of the Society on 1888 January 30.

WILLIAM FRASER DOAK was born at Dreghorn, Ayr, on 1869 May 20. After early education locally and later at Glasgow High School, he proceeded to Glasgow University from which he graduated in 1890. Straight from the university he joined the staff of H.M. Nautical Almanac Office, then situated in Verulam Buildings, Gray's Inn, as a Grade II Assistant. He was destined to spend thirty-nine years in the Office, serving under three Superintendents—J. R. Hind, A. M. W. Downing and P. H. Cowell.

In the days before the general application of calculating machines to computing, the calculation of astronomical ephemerides was a labour demanding the highest degree of arithmetical skill—far more than is the case to-day. Doak possessed the necessary ability and rapidly became a fast and accurate computer, which qualities he retained to the day of his death. In many ways, the work of a computer, especially in the days of logarithms, was very circumscribed, and it offered too narrow a field to a man of Doak's active mind. While continuing to perform his duties in the calculation and preparation of the *Nautical Almanac* with the utmost efficiency, he sought other fields in which his abilities could be utilized; for many years he was engaged in part-time actuarial calculation for various insurance companies. In later years, and especially after his retirement, he built up a personal organization for the supply of astronomical and tidal data to publishers of calendars and diaries; eventually the great majority of firms obtained such information from Doak. At one time also he became interested in survey calculations and became a Fellow of the Royal Geographical Society.

Although he maintained an active interest in astronomy and astronomical navigation, he seems only to have shared in the production of one book, in 1903—*Stars and Sextants*, written in co-operation with Sprigge and Hudson (two other Assistants in the Nautical Almanac Office) and Cox. The alternative title "Star Distance Tables for facilitating the use of Lord Ellenborough's method of correcting the centring and total errors of

sextants at sea" summarizes the contents; although now long forgotten, this little book contains much of practical and theoretical interest.

Even after his retirement from the Office, on reaching the age limit, on 1929 May 20, he continued to work for the Office as an "outside worker". Until a few months before his death on 1943 February 10, he read proofs of the various publications of the Office with a speed and accuracy which were truly astonishing.

Doak was a Scot, of rather a retiring and taciturn disposition; his many interests outside the Nautical Almanac Office did not encourage the formation of intimate friendships with the other Assistants. He was a huge well-made man, considerably over six feet in height, and the legend of the Indian clubs, with which he could keep himself fit and probably, if the truth were told, terrify his younger and less brawny colleagues, still persists.

He married, in 1902, Edith, daughter of Mr. John Mackenzie of Stornoway; she survives him with two children.

He was elected a Fellow of the Society on 1897 February 12.

D. H. SADLER.

CHARLES ROBERT D'ESTERRE was born in London in 1876, and died after a very short illness at Ottery St. Mary, Devon, on 1943 September 11.

His original name, under which he was elected a Fellow on 1895 February 8, was Charles Roberts. His early days were spent at Bournemouth, where he was educated at a private school until the age of sixteen. He then entered the City and Guilds School, London, where he specialised in electrical engineering. Although he adopted engineering as a profession, his greatest interest was in astronomy, and when on the death of his parents he found himself with sufficient private means to follow his own inclination, he gave up engineering and bought a house in Surrey. Here he built and fitted up an observatory and carried on astronomical work for several years. He was especially interested in astronomical photography, and did some excellent work in photographing star fields and regions of the Milky Way.

The Great War of 1914 interrupted his work, and he joined the R.A.S.C., attaining the rank of Staff Sergeant, and was awarded the Military Medal. After a time in Cologne with the Army of Occupation he was discharged and returned to Surrey. His sight and health, however, were not good enough for him to be able to carry on his astronomical work, and he ultimately dismantled his observatory and distributed his telescope, mirrors, and other instruments.

His principal photographic work was done with a 15-inch reflector of 6 feet focal length. The mirror was of excellent definition, and was said to be by Calver. Although the mounting and the drive of the reflector were rather rough, he attained first class results. This mirror he presented with other things to the writer, who in turn has presented it to the Royal Astronomical Society. He also possessed a 30-inch mirror of about 10 feet focal length, which is now at Canberra Observatory.

Two papers by him appeared in the *Monthly Notices*, on the faint stars in the region surrounding the double star cluster in Perseus (Vol. **73**, p. 75, and Vol. **74**, p. 534). d'Esterre dealt with the question of what happens among the faint stars in the background of the Milky Way. He found in this region a Nova (10·5 magnitude when first discovered) and several variables amongst stars down to the 15·5 magnitude which were reached with exposures of 70 minutes. The excellence of the star images showed that d'Esterre's equipment and technique were capable of yielding still fainter stars with longer exposures, and he was asked to take part in the polar sequence work undertaken by Chapman and Melotte (*Mem. R.A.S.*, Vol. **60**, p. 169). He secured for this some plates which gave good images down to 18th magnitude. In d'Esterre's second paper in *Monthly Notices*, Vol. **74**, he claims to have reached the 19th magnitude, a remarkable performance for a 15-inch reflector.

In 1905 he married Katherine Cecilia Doyle, only daughter of Captain B. W. Doyle, sometime British Consul in Brazil. There were no children of the marriage.

J. H. REYNOLDS.

PATRICK JAMES FAGAN, born in 1865 at Comilla, Bengal, belonged to an old Dublin family who had served India for several generations. He entered the Indian Civil Service in 1886, and held many important posts during his thirty-four years service. He was made C.S.I. in 1917 and K.C.I.E. shortly before his retirement in 1923. On his return to this country he took an active part in the work of the Fairbridge Farm Schools, and from 1931 he was also Chairman of the London Society for Teaching and Training the Blind.

Sir Patrick married in 1890 Emily Frances, and they had two sons and a daughter; their younger son died of wounds in 1916 July.

He was elected a Fellow of the Society on 1930 January 10.

MARY THOMASINA LOCKYER was the younger daughter of Mr. Samuel Woolcott Browne. When living in London she and her sister, the late Miss Leigh Browne were students at Queen's College, which had been founded in 1848 by Frederick Maurice, Henry Morley and others to provide education for young women. The two sisters took an active part in social work; they were amongst the devoted band of workers who, under Octavia Hill, strove for the improvement of urban conditions, which were appalling at that time, and for better housing; they also worked in association with Canon and Mrs. Barnett at Toynbee Hall. Being impressed by the lack of provision for the residential accommodation of women students in London, they used a small legacy to rent a house as a hostel for women students. The movement which they pioneered, with the support of their mother and of their friend, Miss Mary Kilgour, led eventually to the foundation of College Hall, for the accommodation of women students at London University. College Hall received at first no official recognition from the University, but this difficulty was later overcome. The present College Hall in Malet Street, which accommodates nearly two hundred women students, was opened by Queen Mary in 1932; it has unfortunately been severely damaged by enemy action.

Lady Lockyer was twice married, first to Mr. Bernard E. Brodhurst, F.R.C.S., the pioneer in orthopædic surgery, and secondly to Sir Norman Lockyer in 1903, the year in which Sir Norman was President of the British Association. After a few years they left London to live in Sidmouth, with which Lady Lockyer had early associations, from visiting her grandparents, Captain and Mrs. Carslake, at the old family house, "Cotmaton". It is of interest to mention that both her grandfathers had fought in the Battle of Trafalgar, Captain Carslake being then a midshipman on Nelson's flagship; later, when both prisoners of war in France, they became firm friends.

Sir Norman and Lady Lockyer built a house on the slopes of Salcombe Hill, with a magnificent view across the bay. A small telescope was set up near the house, but in 1912 Sir Norman decided to build an observatory on the top of Salcombe Hill, where there was an unbroken horizon. This observatory was at first known as the Hill Observatory, but in 1916 the name was changed to the Norman Lockyer Observatory, when the observatory was formed into a corporation to promote its development and to ensure its continuance. Sir Norman and Lady Lockyer in their lifetimes gave considerable endowments as well as valuable equipment to the observatory. Sir Francis McClean presented the twin-telescope used by his father, Dr. Frank McClean, and Sir Robert Mond gave a photographic equatorial. Sir Francis McClean, Captain W. N. McClean and Sir Robert Mond contributed generously to the funds of the observatory. Lady Lockyer bequeathed to the Observatory Corporation real property and made it

her residuary legatee; the funds of the Corporation will benefit appreciably from these bequests.

Lady Lockyer took the keenest interest in all local affairs, and particularly in the preservation of the amenities of the Sid valley. She was honorary treasurer for many years of the Sidmouth Maternity and Infant Welfare Centre. She had the gift of charity in its truest sense.

Lady Lockyer was elected a Fellow of the Society in 1923, a few years after the admission of women to the fellowship became possible. She died on 1943 September 9, after a brief illness, in her ninety-second year. She was buried alongside Sir Norman in the little churchyard of Salcombe Regis, on the summit of Salcombe Hill and overlooking the sea. To the last she led an active life, her mind was undimmed and she took a great interest in all the affairs of the day. She was possessed of a rare and outstanding charm and her many friends will always remember her with deep affection.

H. SPENCER JONES.

SIDNEY BATT MATTEY was born in London on 1886 April 18 and died at Sevenoaks on 1940 December 22.

After taking the degree of B.Sc. he became science master at Shooters Hill School, Woolwich. He was particularly interested in the introduction of astronomy into the school curriculum, and in popular astronomy for adult classes. In addition Mattey took a keen interest in meteor observation.

Mattey was married in 1914 to Miss C. A. Seabrook, science mistress at Conway Road School, Plumstead, by whom he is survived.

He was elected a Fellow of the Society on 1919 February 14.

JAMES EDWARD NICHOLSON was born on 1857 January 21 in London and died on 1943 June 21 in Douglas, Isle of Man. He entered the Army Medical College at Netley in 1879, and in the following year was commissioned as Captain in the Army Medical Service, now the R.A.M.C. His career in the army was varied and interesting, and he served all through the South African War and the Great War.

From an early age he was deeply interested in astronomy, and published numerous pamphlets on astronomical topics, as well as delivering lectures on a variety of astronomical subjects. He maintained a keen interest in scientific thought, and his last work, entitled *Anthropos, or the Problem of Man*, was published a short time before his death.

He married Adelaide Roddam on 1882 December 12, and had two sons and a daughter. His younger son was killed in action in 1916.

He was elected a Fellow of the Society on 1938 March 11.

DOROTHEA KLUMPKE ROBERTS was born in San Francisco on 1861 August 9; she died on 1942 October 5.

Mrs. Roberts was well known as an astronomer in Europe and the U.S.A. She was educated in Germany and later in France, where she was admitted to the University of Paris, and specialised in mathematics. She was the first woman to receive the degree of Docteur-ès-Sciences, her thesis being "L'Étude des Anneaux de Saturne". She then became attachée at the Paris Observatory, working specially on the determination of star places on photographic plates. It was then that she was put in charge of the Charts for the Paris Zone of the Carte du Ciel, and a catalogue of all stars to the 11th magnitude appearing on the plates. The supervision of this work absorbed a major part of Dr. Klumpke's time and energy until 1901. In that year she married a well-known Fellow of the Royal Astronomical Society, Dr. Isaac Roberts, who was one of the pioneers of nebular photography. Dr. Isaac Roberts died in 1904, but Mrs. Roberts

continued the study of astronomical photographs which had been produced with the 20-inch reflecting telescope. In 1929 she issued *The Isaac Roberts Atlas of 52 Regions, a guide to William Herschel's Fields of Nebulosity*. In 1934 came the principal honour she received, when she was elected Chevalier de la Légion d'Honneur.

About this time she gave up her active work and returned to her native land with her sister Anna Klumpke the artist, and took up her residence again in San Francisco. Here she kept in touch with many of her old and new friends, and gave to the University of California a sum of money to establish two prizes, one in Astronomy and one in mathematics. She also contributed $1000 to the Lecture Fund of the Astronomical Society of the Pacific, and a sum of $3000 was left in her will to the American Astronomical Society "to be used in accordance with my wishes".

She was elected a Fellow of the Society on 1926 January 8.

J. H. REYNOLDS.

GEORGE STREET was born at Ruabon (N. Wales) on 1868 October 31 and died at Dunton Rectory near Bletchley on 1943 June 25. He was educated at University College, London, and at Worcester College, Oxford. After spending some time as an assistant master at Merton House School, Southwick, Surrey, he entered the Church and at the time of his death he was Rector of Dunton and Vicar of Hoggeston.

He was a keen and enthusiastic student of astronomy from early boyhood and was always delighted to give a lecture on astronomy or to demonstrate with his telescopes. He did not, however, do any systematic astronomical work.

In 1917 he married Lucilla Mary Digby Russell, by whom he is survived.

He was elected a Fellow of the Society on 1907 January 11.

FRANCIS JOHN WELSH WHIPPLE was born on 1876 March 17, and died at 6 Addison Road, Chiswick, on 1943 September 25. He was the third son of G. M. Whipple, Superintendent of Kew Observatory from 1876 to 1893. He was educated at Merchant Taylors' School and Trinity College, Cambridge, where he graduated as second wrangler. After thirteen years as mathematical master at Merchant Taylors' School he was appointed to the staff of the Meteorological Office, London, where he remained until his retirement in 1939. Here he served first as Superintendent of Instruments, later as Superintendent of the Rainfall Division, and from 1925 to 1939 was Assistant Director of the Office and Superintendent of Kew Observatory.

As Superintendent of Kew Observatory he was specially interested in the transmission of sound through the atmosphere, atmospheric optics and atmospheric electricity. His researches covered a wide field and included contributions to pure mathematics, dynamical meteorology, and many branches of geophysics. Among his most valuable contributions is to be reckoned an article on atmospheric optics in the *Dictionary of Applied Physics*, Vol. III. His researches gained him the degree of Sc.D. of the University of Cambridge.

He served on many Committees and similar bodies. He served for many years as a member of the Council of the Royal Meteorological Society, and was President of that Society during 1936 and 1937. He was editor of the Society's publications for a short time, until ill-health forced him to give up work for a time at the end of 1942. He was for some time a member of the National Committee for Geodesy and Geophysics, and frequently attended the meetings of the International Union of Geodesy and Geophysics.

He was at all times a pleasant colleague, always very ready to help younger men, and many papers published by colleagues owed something to his keen critical faculty.

He was elected a Fellow of the Society on 1931 January 9.

D. BRUNT.

FRANK SCHLESINGER was born in New York City on 1871 May 11, the youngest son of seven children of Joseph William and Mary Wagner Schlesinger. After graduating from the College of the City of New York in 1890, he served for five years as a surveyor and then entered Columbia University as a graduate student. His name will always be associated with the accurate measurement of stellar parallaxes, though his work in this field was by no means his only important contribution to astronomy. Schlesinger's interest in the problems of the accurate determination of star positions by means of photography was aroused by his post-graduate research work at Columbia, where Rutherfurd had obtained photographs of excellent quality. The great possibilities of photography in this field were just beginning to be realised; the thesis submitted by Schlesinger in 1898 for his doctor's degree was concerned with the measurement and reduction of Rutherfurd's photographs of the Praesepe group, and showed unusual skill and ingenuity. He was impressed with the great advantages that photography would offer, provided proper precautions were taken, for the determination of stellar parallaxes; in the following year he published a paper entitled "Suggestions for the determination of Stellar Parallax by means of Photography", which he had read at the Third Conference of Astronomers and Astrophysicists. He referred to the undue neglect of this important field of work, estimating that not more than 25 or 30 stars had known parallaxes which could be relied on to within 0″·05. He outlined the principal precautions that were necessary for determinations of high accuracy to be made by photography; the parallax star was to be placed in the centre of the field and its image was to be reduced to approximate equality with that of the comparison stars by local desensitization of the film—the method of the rotating sector, now generally used, was a later idea of Schlesinger's; all photographs were to be taken with the telescope on the same side of the pier, to avoid effects due to differential flexure, tilt, or possible displacement of the objective, which might be introduced by reversal; distortions of the film were to be guarded against. It was characteristic of Schlesinger to make an estimate of what results were to be expected from such a programme of observation:—

> "According to a conservative estimate, a single observer, working fifteen or eighteen hours a week at the telescope and employing the rest of his time in measuring and reducing, could give us in three or four years the parallaxes of 200 stars with an accuracy hitherto attained for only a score."

Schlesinger was himself to prove that this estimate was not unduly optimistic. But the opportunity did not come immediately. With the inception of the International Latitude Service, two International Latitude Stations were established in the United States, at Ukiah, Cal., and at Gaithersburg. Schlesinger was placed in charge of the station at Ukiah in 1899. He entered into the work of determining the latitude variation and of considering the problems associated with it with the same keenness and insight which he had shown in his treatment of the photographic observations and succeeded in throwing some light on the then obscure z-term. Latitude variation was a subject in which he always retained an interest. Many years later he designed and had constructed a photographic zenith telescope, in which the exposures on the stars and the reversals of the telescope were all performed automatically, at predetermined times, with the aid of a clockwork control. The observer had merely to insert the plate before the observations started and to remove it after they had ended. Schlesinger had promised to lend me this telescope for some latitude variation observations at the Cape; but the discussion by Oort of a preliminary series of observations revealed some anomalies which, I believe, were never satisfactorily accounted for. It is probable that Schlesinger was so fully occupied with matters which he regarded as more important that he was unable to spare the time required for their investigation.

In 1903 Schlesinger was appointed to the Yerkes Observatory, and had at last an opportunity to apply the methods for the determination of stellar parallaxes which he

had previously sketched out. The Yerkes 40-inch refractor was not ideal for the purpose; the size of the instrument and the longish exposures required with a visually corrected instrument, used in conjunction with a colour filter, made it rather inefficient for parallax work. But it sufficed to test the methods. Careful attention was given to the elimination or control of every probable source of systematic error, and it was not long before preliminary results of satisfactory accuracy promised success. In 1905, however, Schlesinger left Yerkes to take up the appointment of Director of the Allegheny Observatory. Brashear had collected funds for a new observatory, and it fell to Schlesinger to plan it and to superintend the move from the old observatory. For a time he became a spectroscopist. Jointly with R. H. Curtiss, he designed and constructed a one-prism spectrograph of low dispersion. The spectroscopes commonly in use at that time were high-dispersion instruments, suitable for the determination of the radial velocities of stars with sharp lines; they were not suited for photographing the spectra of early type stars with broad diffuse lines. The radial velocities of many early type stars were measured and the orbits of a number of spectroscopic binaries were computed. Schlesinger devised a simple method of reducing spectrograms, worked out a convenient method of correcting preliminary elements by least squares, and computed and published extremely useful tables for the true anomaly in elliptic orbits.

But, as opportunity offered, work was continued on the parallax material that had been obtained at Yerkes. In 1906 Schlesinger designed an excellent and efficient long-screw measuring engine, which was made by Brashear. Many similar machines have since been made by Gaertner, and are in use in various observatories. At length, in 1910 and 1911, the results of the work at Yerkes were published in a series of seven important papers in the *Astrophysical Journal*. Every aspect of the determination of stellar parallaxes was discussed with such thoroughness and insight that the papers have justly become classics in this field. The methods of observation, the precautions needed to avoid systematic errors, the methods of measurement and of reduction were all described in detail. Every subsequent worker in this field has closely followed the methods developed by Schlesinger. It was characteristic of Schlesinger in all his work to consider carefully the results achieved in relation to the effort involved; this required a thorough analysis of all the contributory sources of error and an estimation of their magnitudes. Thus, for instance, it might be thought that the parallaxes would have greater accuracy if a large number of comparison stars were employed; Schlesinger showed that if four well-placed comparison stars were selected, the gain in accuracy from measuring a larger number of stars was small. With possibly 2000 or more plates to be measured in a year, this is an important consideration. He also investigated the effect of film distortions and showed that their contribution to the total probable error was small in relation to the other sources of error; thus there was nothing to be gained by following Kapteyn's method, in which plates, after being exposed, were stored undeveloped and exposed again at a second epoch. The method necessarily involves a considerable wastage of material. The convenient method of dependences, devised by Schlesinger, is generally used for the reduction of parallax plates; it has the great advantage of showing at a glance the relative weight of each comparison star.

In 1914, on the completion of the Thaw photographic refractor of 30 inches aperture and 46 feet focal length, Schlesinger started a programme of parallax determinations at Allegheny. It was an ideal instrument for the purpose. Whereas the 28 parallaxes measured at Yerkes had an average probable error of $\pm 0''{\cdot}013$, the first 50 determinations at Allegheny had an average probable error of only $\pm 0''{\cdot}0085$. In the six years that he remained at Allegheny the parallaxes of 365 stars were determined and plates for several hundred additional ones were obtained. Schlesinger also arranged a scheme of co-operation between the seven observatories of Allegheny, Dearborn, Greenwich, Leander McCormick, Mount Wilson, Sproul and Yerkes. It could no longer be said that parallax observations were being neglected, at least in the northern hemisphere, and

results came at a rapid rate. But nothing had been done in the southern hemisphere beyond a few good heliometer determinations at the Cape, and a few photographic determinations of moderate accuracy with the astrographic telescope at the Cape. Schlesinger began to consider how this deficiency in the southern hemisphere could be made good.

The opportunity came after Schlesinger had left Allegheny and taken up, in 1920, the position of Director of the Yale Observatory. The construction of a large photographic refractor for parallax determinations in the southern hemisphere was undertaken. At the end of 1923 he went to South Africa to decide upon a site for the instrument. Kimberley, Bloemfontein, Pretoria and Johannesburg, as well as other places, were investigated. It was finally decided to set up the telescope in the grounds of the University of the Witwatersrand, Johannesburg. He admitted that somewhat better conditions from the astronomical point of view could have been found elsewhere, but he considered also the amenities of life for the staff, in the way of social contacts, cultural activities and educational facilities. The telescope, of 26 inches aperture and 36 feet focal length, was erected in 1925. The whole installation was designed solely with a view to an intensive programme of parallax observations, and, in order to reduce costs, the housing has no dome, the movement of the telescope being limited to about half an hour either side of the meridian. Schlesinger intended the programme of work to continue for ten years in the first place; but this was extended and the work still goes on.

At about that time, as the programme of radial velocity determinations on which the 24-inch Victoria refractor at the Cape had been engaged was nearly completed, I had planned to use the telescope for parallax determinations. The commencement of this programme was somewhat delayed by the appearance of Nova Pictoris in 1925. By arrangement with Schlesinger, the Yale programme of observation comprised mainly the brighter stars and the Cape programme mainly the stars of large proper motion. The Yale telescope, efficient in operation, observing bright stars which need only short exposures, and working under the favourable atmospheric conditions of Johannesburg, has obtained parallax plates at a record rate. Some fields are measured by the small staff in Johannesburg, but the majority of the plates have been sent to Yale for measurement there. In volume 8 of the *Yale Observatory Transactions*, the measures, reductions and results for 851 stars were given, and volume 15, pt. I, which has recently appeared, contains another 472. Material for many further fields has been measured or is in course of measurement. The Cape Observatory has published 1200 parallax results, so that the southern hemisphere has made good the deficiency in this field of work.

The determination of stellar parallaxes by spectroscopic methods would not have been possible without a sound basis of trigonometrical parallaxes by means of which the spectroscopic criteria could be calibrated. The spectroscopic methods have added considerably to our knowledge of stellar parallaxes and are particularly valuable for the more distant stars for which the parallaxes are comparable with the probable errors of the trigonometrical determinations. In 1924 Schlesinger published a *General Catalogue of Parallaxes*, containing the parallaxes of 1870 stars. A second edition, published in 1935, contained the parallaxes of 7534 stars; for about 4000 of these, trigonometrical determinations were available. It must have been very gratifying to Schlesinger that his work had in so short a time produced so rich a harvest. This catalogue was not a mere compilation; it involved a very comprehensive and searching investigation into the systematic and accidental errors of the various series. The data in the catalogue have been subjected to various tests which have fully confirmed the errors assigned by Schlesinger and the systematic accuracy of his final system. The catalogue is a valuable work of reference.

A further reference work of great value compiled by Schlesinger is the *Bright Star Catalogue*, the latest edition of which contains all the important data known in 1940 January relating to all stars brighter than 6·5 visual magnitude and to some fainter ones.

The data include name, DM and GC numbers, position for 1900·0 and annual variations, magnitude, spectral type, proper motion, parallax, radial velocity and notes about visual and spectroscopic binaries, variables, cluster stars, composite spectra, etc.

Another great programme that occupied much of Schlesinger's time for many years was the determination of star positions by means of wide-angle cameras. The purpose of this work was to relieve meridian observers of the burden of determining the positions of a large number of faint stars and to determine them more rapidly, more economically and more accurately by photography. This would leave meridian observers free to concentrate on the brighter stars. In such a programme a wide-angle camera, with plates covering a large field, is necessary in order that a sufficient number of the brighter stars shall be available on each plate for use as reference stars. Preliminary observations were carried out whilst Schlesinger was at Allegheny, and his conclusions were summed up in the following paragraph (*Allegheny Publ.*, **3**, No. 9):—

"The present objective would be well adapted for compiling zone catalogues of faint stars similar to those of the AG. The latter now extend from 80° N. Dec. to 18° S. Dec., a work that has required the co-operation of 16 observatories during a considerable number of years. . . . The original plans for the Gesellschaft Catalogue included the repetition of the observations after the lapse of about half a century. This time is now approaching in the case of some of the northern zones. . . . The repetition of all the northern zones, if carried out photographically by means of a doublet, would be a task well within the powers of a single observatory. Moreover, there would be a very considerable gain in accuracy."

At Allegheny he re-observed photographically two zones of the AG, $+50°$ to $+55°$ and $-2°$ to $+2°$. But he was appointed Director of the Yale Observatory before the measurement was completed and the results were published in the *Yale Observatory Transactions*. Further zones were photographed in quick succession, and the measures, discussion, and publication completed with remarkable speed—a tribute to the efficiency of Schlesinger's methods and organisation. Having gained initial experience with plates covering 25 square degrees ($5° \times 5°$), he went still further, and, with a new lens of 5·6 inches aperture and 80 inches focal length, photographed fields of 10° in Dec. by 14° in R.A. (140 square degrees). The plates for these large fields were of plate glass, measuring 48 cm. by 58 cm., with a thickness of 6 mm. A special measuring engine of large size was designed and constructed for measuring these plates. It was enclosed in a glass case and protected from the heat of the illuminating lamps and the measurer's body. A single plate would contain several hundred stars and the measurement in one co-ordinate might take two or three days. Six zero dots were marked on the plate and measured every $1\frac{1}{2}$ hours as a control. Similar observations were also undertaken with a wide-angle camera mounted on the Yale telescope in Johannesburg; in this camera a lens of 5 inches aperture and the same focal length, with a field of 112 square degrees, was used. The greater efficiency of this lens enabled the time of exposure to be reduced from 12 minutes to 6 minutes. The plates measured 43 cm. square and were 6 mm. thick. As an instance of Schlesinger's ingenuity it may be mentioned that he noticed that these large plates, even though 6 mm. thick, were almost always concave on the coated side. He therefore inserted in the plate carrier an accurately flat thick duralumin plate, which was firmly pressed at the back against the centre of the glass. The effect of this was usually to make the plates flat; it could not, in any case, make them less flat.

The reduction and discussion of plates of such large area and the investigation of optical distortions raise a number of intricate points. These were dealt with by Schlesinger with consummate skill and the final places have a high degree of accuracy. In the latest catalogue to be published, the probable error of a catalogue place is $\pm 0''·105$ both in R.A. and in Dec. The list of catalogues published makes impressive reading, and is as follows:—

Yale Trans. Volume	Zone of Dec.	No. of Stars	Year of Publication
	° °		
3	+ 1 to + 2	1275	1925
4	+50 „ +55	8359	1925
5	− 2 „ + 1	5833	1926
7	+55 „ +60	7727	1930
9	+25 „ +30	10358	1933
10	+20 „ +25	8703	1934
11	−10 „ −14	8101	1939
12	−14 „ −18	8563	1940
	−18 „ −20	4553	1940
13	−20 „ −22	4292	1943
	−27 „ −30	9455	1943
14	−22 „ −27	15110	1943

Amongst Schlesinger's other contributions to astronomy, mention may be made of his investigation of the errors in photographic positions introduced by photographing through the glass. He found that with plates of ordinary glass the mean error was of the order of 0·001 mm. He therefore suggested the determination of proper motions by differential measurements of plates taken at two epochs, the later plates being photographed through the glass and the two plates being placed for measurement with their films in contact. This method has since been widely used. His investigation of irregularities in atmospheric refraction by photographing star trails showed the presence of slow oscillations having a period of the order of one minute and a double amplitude of about 1″. Two neighbouring stars were similarly affected up to distances of at least 20′.

Schlesinger took a prominent part in the formation of the International Astronomical Union after the last war and in guiding its subsequent policy. He was Vice-President for the period 1925–1932, and President for the period 1932–1935, which ended with the meeting of the Union at Paris in 1935. His sane and balanced judgment and wise counsels were of great help to the Union. Schlesinger received the honorary degree of Sc.D. from the Universities of Pittsburgh and Cambridge. He was elected an Associate of the Royal Astronomical Society in 1914 and was awarded its Gold Medal in 1927. He was awarded the Valz Prize of the French Academy in 1926 and the Bruce Medal of the Astronomical Society of the Pacific in 1929. He retired from the Directorship of the Yale Observatory in 1941 June, but, though he suffered much ill-health in his last years, he continued to work on the zone catalogues to the end. He died on 1943 July 10 at his home in Lyme, Connecticut.

Schlesinger was twice married, in 1900 to Eva Hirsch of Ukiah, who died in 1928, and in 1929 to Mrs. Philip Wakeman Wilcox. He is survived by his widow and one son by his first marriage, Wagner, of the Fels Planetarium, Franklin Institute, Philadelphia.

Schlesinger was a man of many friendships, much respected and trusted by all.

[By courtesy of the Editors of *The Observatory*, from which the above is largely reprinted.]

H. SPENCER JONES.

REPORTS ON THE PROGRESS OF ASTRONOMY

SOME PROBLEMS OF THE OUTER LAYERS OF THE SUN

During the quarter of a century ending about 1930 the dominating ideas in research on the outer layers of the Sun were provided by the theories of formation of the continuous spectrum and Fraunhofer lines, based on consideration of the mode of transfer of radiation through idealized static atmospheres, which were taken to be in a state not far removed from thermodynamical equilibrium. It was realized all along that to regard the atmosphere of the Sun or any other star as a static entity is only an approximation to the truth (although the point was sometimes rather glibly passed over), but about 1930 it was beginning to be found that various factors associated with deviations from a purely static equilibrium could no longer be ignored, and research began to take a new trend. For instance, the conception of "turbulence" was introduced, first in connection with the solar chromosphere (McCrea, Menzel), and later for stellar spectra in general, to deal with multiplet intensities and the "curve of growth" (Struve). Approaching the question from a very different direction, radio research had encountered great variations of ionization in the upper layers of the Earth's atmosphere, and these had been found to be connected closely with bright eruptions (chromospheric eruptions, or perhaps more accurately "flares") on the Sun. The mechanism involved here is still obscure, but it is generally believed that these eruptions produce a large amount of ultra-violet light, of a wave-length far shorter than we can observe directly through the Earth's atmosphere, possibly in the hydrogen Lα line. It is fairly certain that sometimes there is a great excess of high frequency radiation emitted from the Sun's surface, relative to what there would be with a black-body distribution of radiation energy, and there is reason now to believe that at all times there is some excess. On account of the utilitarian importance of radio communication, the purely observational watch which has been kept on the surface of the Sun for many years as a matter of routine, by means of spectroheliograms, sunspot counts, etc., has been supplemented more recently by examination of many of these eruptions, chiefly by means of the spectrohelioscope. Another field of observation has also been opened up by the application of cinematography to the solar prominences (Lyot, McMath). It was also soon after 1930 that another line of attack was initiated by Lyot, who by a very careful technique succeeded in photographing the corona in full daylight, so that observations made hitherto only occasionally at total solar eclipses may now be made frequently, if not daily. The importance of Lyot's achievement has recently been enhanced by Edlén's identification of the source of the bright line spectrum of the corona with extremely highly ionized iron, nickel, etc. The 1930's saw as well a great improvement in the photometry of Fraunhofer lines, in which a considerable number of workers took part, and the results have led to a more critical scrutiny of the standard theory. Then interest has been reawakened in the solar granulation (Plaskett, ten Bruggencate), a permanent feature of the Sun's surface, very difficult to observe in as detailed and accurate a manner as we desire, while recently the question of turbulence has been treated theoretically (Unsöld, Eddington), with emphasis on the peculiar and dominating rôle played by hydrogen ionization in the outer layers of the stars. In short, there has been during the past ten or twelve years a marked swing in solar research away from the earlier static models of theory to a closer examination of deviations from equilibrium, and in general to the more dynamical aspect of the problem. The outstanding and indispensable book on the subject is Unsöld's *Physik der Sternatmosphären* (Berlin,

1938), which, if we may use war phraseology, describes authoritatively and in full detail the older campaign and the beginning of the new offensive down to 1937.

The Standard Models

(a) *The Continuous Spectrum.*—For the purposes of this article, which has approached the subject by way of those problems encountered during work on spectrograms taken at the total solar eclipse of 1940*, it is perhaps well to begin with certain difficulties in the standard theory of Fraunhofer lines. For any given wave-length the theory yields the intensity within an absorption line at any point of the Sun's disk, provided we know three quantities as functions of optical depth τ, viz. the intensity of emission in the continuous spectrum $\mathcal{J}_T$, the ratio of line absorption coefficient to continuous absorption coefficient η, and the fraction of atoms de-excited by collisions of the second kind ϵ. In this way, for example, the theoretical variation of a Fraunhofer line from the centre of the Sun's disk to the limb may be computed. The best plan is to deduce $\mathcal{J}_T$ from the observed darkening to the limb in the continuous spectrum, and for this there are two requirements: (i) sufficiently accurate observations of the limb darkening, (ii) an accurate method of deducing $\mathcal{J}_T$ from these. Unfortunately neither requirement is at present satisfied with the necessary precision, at least not if it is desired to make anything more than a rough study of the course of line depth for points on the solar disk near the limb. There are three sets of limb darkening measures available, those of Abbot †, of Moll, Burger and van der Bilt ‡, and of Raudenbusch. § Abbot and Raudenbusch both give their measures to four significant figures, but it is clear from the interagreement of the three investigations that we are still uncertain of the true values to anything better than one per cent. There are two possibilities: (*a*) that the limb darkening varies slightly with time, an unwelcome complication that cannot, however, be ruled out as yet; (*b*) that the measures are not yet sufficiently accurate. With regard to the latter, it may be remarked that observations should if possible go closer to the limb than $\cos\theta = 0{\cdot}25$; that no satisfactory method of correcting the measures for scattering in the sky and scintillation has yet been worked out (the use of an error or exponential law for the scattering is insufficient); that instrumental scattering cannot in general be neglected (it is undesirable to use a cœlostat or other mirrors in the optical train before the solar image; observers should in general emulate Lyot); and that if four-figure accuracy is to be achieved it is probably necessary to use a spectrograph of very high resolving power, to eliminate as far as possible all Fraunhofer lines, since these have their own centre-limb variation, not the same as that of the continuous spectrum. Even apart from the considerable observational difficulties we still encounter an objectionable amount of uncertainty in the purely numerical procedure of deducing $\mathcal{J}_T$ from the measures, as may be seen by comparing work by Plaskett || and by Busbridge. ¶ The latter's method, more elegant mathematically, gives a $\mathcal{J}_T$ at $\tau = 0$ too low to be physically permissible. The fundamental trouble is that a small change in the limb darkening produces a large change in the course of $\mathcal{J}_T$, and the numerical procedure will doubtless be quickly improved when more reliable observations are available.

(b) *The Fraunhofer Lines.*—At present one of the best ways of testing any theory of formation of Fraunhofer lines is to compare at some fixed wave-length the theoretical and observed line intensities as they vary from centre to limb of the Sun, *i.e.* with θ, the angle between the line of sight and the normal to the Sun's surface. It is generally

* *M.N.*, **102**, 134, 140, 1942; **103**, 173, 1943.
† *An. Ap. O. Smith. Ist.*, 4, 229, 1922.
‡ *B.A.N.*, 3, 83, 1925.
§ *A.N.*, **266**, 301. 1938.
|| *M.N.*, **101**, 3, 1941.
¶ *Ibid.*, 26.

supposed that the same parameter distributions should serve all lines equally well, irrespective of the line strength. On this basis the line behaviour is fixed if the line intensity r at some given θ, generally $\theta = 0°$, is known, and the wing of a strong line should show the same centre-limb behaviour as the centre of a weak line at equal r ($\theta = 0°$), if sufficiently precise measures can be made. Houtgast's work on a number of strong lines * shows that this is only approximately true, for not all of his lines show in the wings the slight strengthening to the limb which from Miss Adam's measures appears characteristic of the majority of weak lines.† Hence a complete theory will have to study lines individually, or at least group them according to their atomic source, neutral iron, singly ionized calcium, or whatever it may be. The 1940 eclipse observations pushed measures to much smaller values of $\cos\theta$ than is possible in full daylight. It was found necessary, even in these favourable circumstances, to reduce the measures by a differential process, in order to get rid of contamination by the bright line chromospheric spectrum. The results showed that at a place only about 1″ within the limb the weaker lines of the Fraunhofer spectrum persist with about the same strength as at the centre of the disk, while stronger lines are weakened. With the possible exception of hydrogen lines, no emission lines were detected at this point. In trying to fit these results to the computed line depths for some standard model, a number of difficulties were encountered. Two models, viz. a pure absorption model and the Schuster-Schwarzschild model, were found to give the slight line strengthening found by Miss Adam, but the results were sensitive to the distribution of $\mathcal{J}_T$ with τ, leading to the conclusion, already stated, that our knowledge of $\mathcal{J}_T$ is at present insufficiently accurate. However, quite apart from this, neither of these models is adequate by itself, because the centres of strong lines at the middle of the Sun's disk are much blacker than the pure absorption model will allow, while the S–S model predicts that weak lines will have changed to emission well within the limb, at places where they are observed to be still in absorption. Some of the difficulty may be overcome by taking a model which is a combination of these two—that is, pure absorption layers to form the bulk of the atmosphere, and on top a fairly thin pure scattering layer, lying above the levels at which continuous spectrum is formed. Even this will not agree with the observed limb spectrum, for which it was found necessary to introduce a fresh idea, that the Sun's surface is not perfectly smooth and that the roughness creates a difference between the apparent and effective values of θ near the limb. Further reference is made to this point later. The result that most of the Sun's atmosphere forms Fraunhofer lines by pure absorption is a departure from the pure scattering ($\epsilon = 0$), or nearly pure scattering ($\epsilon = 0{\cdot}1$) models which have been more often favoured of recent years. In this connection it is of interest to remark that in his recent work on the spectrum of the Bo star τ Scorpii, Unsöld ‡ employed a pure absorption model, although of course conditions on this star are rather far removed from those on the Sun.

(c) *Non-Coherent Scattering.*—We see that the practical application of standard theory is still not easy and straightforward, even ignoring complications such as those referred to in our introductory paragraph. There is in addition the question as to whether scattering is coherent or non-coherent, whether atoms re-emit quanta at precisely the same wave-length as they are absorbed, or not. As Eddington pointed out long ago, the whole validity of Fraunhofer line theory depends on the tacit assumption that the emitted and absorbed radiations are of the same frequency—in other words, that scattering is coherent. Failing this, not only has the theory to be reconsidered completely from the start, but also in its general form it becomes extremely complicated and difficult. Houtgast (*loc. cit.*) maintains that his observations of strong Fraunhofer

* "The Variations in the Profiles of Strong Fraunhofer Lines along a Radius of the Solar Disc", Dissertation, Utrecht, 1942.

† *M.N.*, 100, 595, 1940.

‡ *C.D.A.L. Bulletin*, 9, summary of *Z. Ap.*, 21, 1, 1941.

lines cannot be satisfactorily explained without non-coherent scattering, but in reply to this it may be argued that there are so many possible model atmospheres that the capabilities of coherent scattering have not yet been fully explored. Indeed, in so far as Houtgast's observations are consistent with Miss Adam's observations of weak lines, they can be satisfied, as mentioned in the preceding paragraph, by a model using only coherent scattering. A decision between coherent and non-coherent scattering cannot easily be reached on purely astronomical grounds, and it appears better that it should be made by physicists, either theoretical or experimental, who have fewer complications with which to wrestle. There is a certain amount of laboratory evidence, quoted by Houtgast, suggesting that scattering may be non-coherent.

"Turbulence"

(a) *Velocities from Line Widths.*—In the ideal atmosphere of ordinary theory the only motion is that of individual atoms; they move with a Maxwellian distribution of velocities determined by the temperature of the layer in which they find themselves. At the outside of a star, for instance, the temperature is T_0, related to the effective temperature of the star T_e (defined by the rate of flow of radiation out of the star as a whole), by the approximate relation $T_e^4 = 2T_0^4$. Descending from the outside to the interior of a star, there is a steady increase of temperature, determinable to a limited extent in the outer layers by a study of the limb darkening. Departures from the theoretical Maxwellian velocity distribution have of recent years been rather loosely termed "turbulence", irrespective of the exact nature of the atmospheric motions, and ignoring the fact that there is an infinite set of possibilities, once deviation from a static atmosphere in thermodynamical equilibrium is admitted. For example, (1) the star's atmosphere may be in a state of macroscopic motion, shared by all material whatever its atomic constitution, while individual atoms retain their normal thermal velocities, so that within any fairly small volume sharing in the macroscopic motion the velocity distribution remains Maxwellian. Or (2), the motion may be atomic only, but greater than Maxwellian, and resolvable into the ordinary thermal velocity plus an additional velocity which is the same for all atoms. Or (3), there might be a Maxwellian distribution corresponding to a higher temperature than that of the radiation traversing the atmospheric layers concerned. (In the opinion of some workers, including the writer, it is misleading to apply the word turbulence to this case.) These are merely three simple instances; the actual state of affairs might be a kind of complicated mixture of all three, or of these with other possibilities. When using the word turbulence it is therefore essential that the type of motion envisaged should be defined in more detail, particularly since the word is widely used both in other sciences such as hydrodynamics and in everyday life. It should be noticed that the computed depth of a Fraunhofer line is not necessarily the same for motions of types (1) and (2), even though the average velocity in the line of sight of the atoms concerned is the same in both cases. The residual intensity of a very strong line is much more quickly raised by velocities of type (1) than of (2), if the light in which observations are made comes from a big enough surface of the star. The "curve of growth", on the other hand, is affected by turbulence of type (2), but not by (1). Allen has designated (1) as "large scale turbulence", and (2) as "small scale turbulence".*

In the early work of McCrea and of Menzel on chromospheric turbulence the motions considered were those of types (1) or (2), with an average velocity in the neighbourhood of 15 km./sec., and their views have held the field until recently. The 1940 eclipse measures, on the contrary, show that in the low chromosphere the rare-earth lines are so narrow that the average velocity in the line of sight of these atoms is less than 2 km./sec. Other elements, such as iron, titanium, chromium, also give narrow lines in the few

* *Ap. J.*, 85, 165, 1937.

favourable cases where these can be found free of serious blends and where the corresponding absorption line in the ordinary Fraunhofer spectrum is quite weak. The latter condition is necessary to minimize "self-reversal", which in these particular spectrograms appeared to be due more to superposed Fraunhofer spectrum than to self-reversal in the true sense. The observed line widths can be satisfied within the errors of observation by assuming a kinetic temperature about 30,000°, and any macroscopic or turbulent motion which is the same for all atoms cannot be greater than about 1 km./sec. in the line of sight. It is difficult to escape from this conclusion, for spectrum lines can hardly be observed to be narrow if they are in fact broad; the whole tendency of observational error, with the exception of Eberhard effect, is in the opposite direction. Accepting the results, a host of interesting queries present themselves. For example, what is the relation of these atomic velocities to the state of excitation and the mechanical support of the chromospheric material? What mechanism maintains the velocities? The results refer to the low chromosphere: what happens as we proceed to the high chromosphere and thence into the corona? Does the velocity distribution remain more or less Maxwellian, with an increasing kinetic temperature, at least until the inner corona is reached? The problem merges into that of the corona, to which fuller reference is made later in this article.

Turning in the other direction, to layers below the chromosphere, the question of atomic and other motions becomes more complex, partly because we have to rely to a great extent on measures of the profiles of absorption, and not emission, lines. The direction of motion given by such measures as these is vertical, if observations are made near the centre of the disk, whereas chromospheric measures give horizontal velocities. For the present we must assume that the velocity distribution is independent of direction, although for some types of motion this is not necessarily the case. Some of the difficulties of the problem appear in Allen's paper (*loc. cit.*), and as far as concerns his own observations of weak or moderately weak lines, he finds it impossible to decide between his "large scale turbulence" and "small scale turbulence", the most probable velocity in the line of sight being 1·8 km./sec. in the first case and 1·5 km./sec. in the second. Later work by ten Bruggencate * appears to have considered only "small scale turbulence", for which he obtains the same velocity as Allen. The present writer's measures of the residual intensities of a few very strong lines † can be taken as pointing to the second of Allen's alternatives rather than the first, but this evidence is not entirely convincing, because line centres may be filled in by other mechanisms. For instance, Dempster ‡ has computed that about half the residual intensity observed by Shane in the D lines can be caused by fluorescence. It is better therefore to take the observed residual intensities of strong lines to indicate only an upper limit of between 1 and 1·5 km./sec. for the average velocity in the line of sight of "large scale turbulence", which agrees satisfactorily with the eclipse chromospheric observations. The atmospheric level chiefly involved in the formation of these residual intensities must be not far from the low chromosphere. The present evidence then is that any motion in the solar reversing layer and lower chromosphere which can properly be called "turbulent", *i.e.* which is common to all material and not Maxwellian in distribution, does not exceed an average velocity of about 1 km./sec. in any one co-ordinate.

Turbulence in the reversing layers of a number of stars has also been estimated by Struve and others from the "curve of growth". This must refer to "small scale turbulence". According to Struve and Elvey the three stars 17 Leps, ϵ Auri and α Pers show "turbulence", while others such as α Cygn, α CMaj and α Lyra do not. § In his recent work on τ Scor, Unsöld finds that the "turbulent velocity" must be less than 2 km./sec.

* *Z. Ap.*, **18**, 316, 1939. † *M.N.*, **97**, 552, 1937.
‡ *Ap. J.*, **96**, 295, 1942.
§ Unsöld, *Physik der Sternatmosphären*, 269, 1938.

A complication when we are using Fraunhofer lines for determining these velocities is that weak lines originate lower down in the atmosphere than the central regions of strong lines, and it is by no means sure as yet that the motion, whether macroscopic or microscopic, is exactly the same for all layers. Our present very imperfect knowledge suggests that on the Sun there may possibly be a diminution of average atomic velocity from the corona down to a minimum at some level, as yet unspecified, which may or may not be above the reversing layer. In the case of peculiar stars such as those showing bright lines, or those with two or more absorption spectra, at present interpreted as arising in gaseous shells, conditions must be much more complicated than on the Sun.*

(b) *The Solar Granulation.*—Observationally the investigation of turbulence on the Sun's surface is limited to what we can see at the photosphere and higher levels. Apart from spectrum measurements based on line profiles, or on "curves of growth", it is possible to study the solar granulation, which, although a permanent feature of the Sun's surface, has been for many years a neglected phenomenon. The work of H. H. Plaskett † redirected attention to the subject and has been followed by an attack on the problem by ten Bruggencate and his collaborators at Potsdam.‡ According to the latest figures, the average diameter of the granules is 1″ to 2″, rather smaller than earlier estimates of about 5″, their average variation in brightness is ±9 per cent., and their mean lifetime is about three minutes. Owing to their transient nature and to the fact that scintillation in a telescope image is much greater by day than by night, the granules are very difficult to observe in detail and it is unlikely that the last word on the subject has yet been said. However, merely from its general appearance it is difficult to avoid the conclusion that the granulation is a product of turbulent motion of some kind, even though the details of the mechanism remain unknown for the present. Reliable observations of line of sight velocity changes in an individual granule during its lifetime would be of the greatest importance, but the task presents a most formidable technical problem and has so far not been attempted. Unsöld has suggested that visual observations might achieve what photography cannot, but the expected Doppler shifts are only of the order of 1 km./sec., a small velocity to be detected and measured quickly by eye.

Rather unexpectedly it was found necessary to appeal to the granulation in order to explain the 1940 eclipse observations of the solar spectrum immediately within the limb. The characteristic features of this limb spectrum are that, relative to the centre of the disk strong lines are weakened, but weak lines persist practically unchanged, while the transition from Fraunhofer to chromospheric emission spectrum at the limb itself is extremely sudden. It was found impossible to explain these points by any ordinary theoretical model whatever, and eventually it was supposed that associated with the granulation is a roughness of the Sun's surface, which need not be very great, but which near the limb reduces the effective value of θ, the angle between the line of sight and the normal to the surface. Quantitative estimates can be made only rather crudely, but the data we have at present support this explanation of the eclipse observations. For the necessary computations an average granule diameter of about 5″ was chosen, with an average separation of 10″ from granule to granule. These quantities, according to the more recent figures quoted above, are rather too great, but in the rough approximation used the effect on the observed spectrum depends only on the shape of the granules and not on their absolute size. If anything, a reduction of the assumed scale of the granulation by a factor or two or three improves the agreement with observation, for the necessary (large scale) average velocity in the line of sight is then under 1 km./sec. If the roughness of the Sun's surface has to be taken into account when studying the Fraunhofer spectrum at the limb, it must also be considered when deducing

* Struve, *Ap. J.*, **95**, 134; 1942.

† *M.N.*, **96**, 402, 1936.

‡ ten Bruggencate and Grotrian, *Z. Ap.*, **12**, 323, 1936; ten Bruggencate, Grotrian and v. d. Pahlen, *Z. Ap.*, **16**, 51, 1938; ten Bruggencate, *ibid.*, 374.

the distribution of continuous emission with optical depth from limb darkening observations. This point has not yet been investigated.

(c) *The Hydrogen Convection Zone.*—Below the reversing layer we enter a region of which only the uppermost layers are accessible to observation, but which according to recent theories may play a very fundamental part in the structure of the whole visible atmosphere. The question of convection on the Sun has from time to time been examined by a number of workers, from K. Schwarzschild (1905) onwards, and has been related to the solar granulation theoretically by Siedentopf *, and more observationally by H. H. Plaskett.† It was, however, left to Unsöld ‡ to point out the dominating rôle which the ionization of hydrogen plays in this matter, and his work has more recently been extended by Eddington.§ Except in broad outline the question is decidedly complicated; the theory, although very attractive, is not without its difficulties, and quantitative estimates are still rather uncertain. It is found that an atmosphere in ordinary radiative equilibrium is unstable, and that convection must be present wherever hydrogen is found partly ionized, which means in a layer near the surface of all but the hottest stars. According to Unsöld the hydrogen convection zone on the Sun is of the order of 500 km. deep, extending downwards from a level about $\tau = 1{\cdot}6$, and the convection velocities are about 1 km./sec. Eddington maintains that the instability of the radiative equilibrium model is even greater than Unsöld has supposed, that most of the energy transport in the convection zone is by ionized hydrogen being brought towards the surface and neutral hydrogen being carried in return towards the interior, that there will in consequence be an excess of high frequency radiation in the photosphere, and that in the case of a Cepheid variable a time lag will be introduced into the star's radiation output, explaining the well-known and hitherto obscure phase difference between the light curve of a theoretical pulsating star and that of an actual Cepheid. The dominance of hydrogen in this theory is due in part to its relatively high ionization potential, but chiefly to the fact that it is supposed to be by far the most abundant constituent of a normal stellar atmosphere. However, according to Unsöld's recent paper on τ Scorpii, helium is almost as abundant as hydrogen, at least in that particular star, and presumably a similar convection mechanism will work with helium, at both stages of ionization, which would carry the convection zone considerably further down into the star than would hydrogen alone. It appears that conditions at the surfaces of stars of all spectral types may be much further from thermodynamical equilibrium than was imagined a few years ago. In particular, there may be much more very short wave-length light emitted than was formerly believed possible, and there may be a considerably higher state of ionization also. An accumulating amount of independent evidence supports the first of these possibilities in the case of the Sun. Hunter || recently listed the following: the excitation of helium in the chromosphere and the prominences, the production of the "twilight flash" spectrum of N_2^+ in the Earth's upper atmosphere, the ionization of carbon monoxide and nitrogen molecules in comets' tails by solar radiation, and the production of D-region ionization in the ionosphere by bright eruptions on the Sun, while Edlén's identifications for the emission lines of the coronal spectrum demand a source of extremely high excitation in the Sun's atmosphere. With regard to the second point it has been known for many years that lines of comparatively high excitation are much stronger in the spectra of cool stars than equilibrium considerations permit (the Russell-Adams effect), while more recent work by Spitzer ¶ has emphasized how extremely remote from thermodynamical equilibrium are conditions in the outer layers of the M-type giants.

* *A.N.*, 255, 157, 1935. † *Loc. cit.*
‡ *Physik der Sternatmosphären*, 379, 1938.
§ *M.N.*, 101, 177, 1941; 102, 154, 1942. See also M. Schwarzschild, *M.N.*, 102, 152, 1942.
|| *Nature*, 150, 756, 1942.
¶ *Ap. J.*, 90, 494, 1939.

The Corona

Brief reference has already been made to Edlén's work on the bright line spectrum of the corona. Circulation of his publications * has been much delayed by the war, but a recent article by Swings †, which may prove more accessible to English-speaking readers, gives a very full summary and discussion of the results.

As is well known, the identification of the source of the bright line spectrum of the corona has been for very many years one of the outstanding unsolved puzzles of astrophysics. Since Bowen's work on the bright line spectra of the nebulæ, ten or more years ago, the general conjecture has been that the "coronium" lines would prove to arise from forbidden transitions in the spectra of some of the more abundant elements, but until now all attempts at identification have been doomed to disappointment. The fundamental clue to the present solution was provided by Grotrian, who in 1939 remarked that two term separations of *Fe*X and *Fe*XI respectively, which had been determined by Edlén from laboratory measures in the extreme ultra-violet, coincided within experimental error with the wave numbers of the two coronal lines λ 6374 and λ 7892. Following this up, Edlén came upon two other similar coincidences with measured term separations until then unpublished, in *Ca*XII and *Ca*XIII. There was thus a suggestion of a very high level of ionization for the atoms giving the coronal bright lines, far higher, indeed, than has hitherto been proposed for any lines in any celestial spectrum, whether from star or nebula. It was found possible to proceed further by utilizing not observed, but calculated, term separations in these and similar spectra, by means of extrapolation along iso-electronic sequences, as for instance the sequence *Al*I, *Si*II, *P*III, *S*IV, . . . *Fe*XIV, *Co*XV, *Ni*XVI. Attention was concentrated on those elements of high cosmical abundance which would give, in the spectrum region normally covered by astronomical observations, forbidden lines of the kind sought. Of these elements, iron and nickel proved the most important. The search was finally rewarded by discovering coincidences with coronal lines in the forbidden spectra of *Fe*X, XI, XIII, XIV, XV; *Ni*XII, XIII, XV, XVI; *Ca*XII, XIII, XIV; *A*X, XIV; leaving only four of the corona's emission lines unaccounted for, of which two are very faint.

Were this all, a critic might complain that, with the uncertainties of experiment and calculation in view, the chances of fortuitous coincidences are too high. Edlén's case is, however, much stronger than this; the selection of lines is by no means haphazard, and good reasons can be given why these and not other lines are observed. Among these are: (1) The identified lines are all of the same general character, viz. arising from forbidden transitions between sub-levels of the ground states of the atoms concerned. (2) Iron and nickel between them account for nine-tenths of the total line emission in the corona. (3) If a line of iron is observed, the homologous line of nickel, which with the same electron configuration is at two stages of ionization higher than the iron, is also found, provided it falls within the range of spectrum accessible to observation. (4) The relative intensities of the lines can be roughly computed, using for example the abundances of elements as found in meteorites and an electron temperature for the corona of 250,000°. While the agreement with the observed intensities is not particularly close, the observed and calculated values are of the same order, which is probably as much as can be expected at present. The data for the calculation are only approximate and the observed intensities are variable. (5) *Fe*XII and *Ni*XIV are absent because the predicted forbidden lines have too low an intensity. *Fe*XV, XVI, XVII and the corresponding stages of *Ni* are absent because the relevant energy level is single, so that lines of the character sought cannot be produced. *Fe*XVIII, *Ni*XX and higher stages are not expected because the level of ionization is too great. With regard to other common elements

* *Arkiv Mat. Ast. och Fysik.* **28**, B, No. 1, 1941; *Z. Ap.*, **22**, 30, 1942.
† *Ap. J.*, **98**, 116, 1943.

Swings states that "no forbidden or permitted line of *Si*, *Mg*, *C*, *N*, *O*, or *Ne* is possible in the corona under the actual ionization conditions". Certain other elements, such as *K*, are not expected to show lines on account of low cosmical abundance.

There is accumulating evidence that the kinetic temperature of the corona is extremely high, relative to the ordinary effective temperature of the Sun's surface, so that the level of ionization in Edlén's identifications need no longer be regarded as fantastic, as it might have been a few years ago. As far back as 1934, Grotrian pointed out that the Fraunhofer spectrum of the corona, believed to come to us by scattering from free electrons, shows progressively more washed-out lines as one proceeds from outer to inner corona, until in the region where the emission lines are found the Fraunhofer lines have practically vanished and there is only a continuous spectrum left. He interpreted this as due to increasing electron velocities, and on the basis of his observations suggested an average electron speed in the inner corona of about 4×10^8 cm./sec. (which in equilibrium would correspond to a kinetic temperature 350,000°). Since Shane's observations at the 1932 eclipse it has been known that the strong green line at λ 5303 is of the order of 1 A. wide, although the natural width of this and the other emission lines is negligible, on account of being "forbidden" lines. The line broadening is practically certainly due to Doppler effect, even though the precise nature of the velocities is still unknown. Lyot's measures * fully support this supposition, and show that the velocity component in the line of sight has very nearly a Gaussian distribution, suggesting that the space distribution may be nearly Maxwellian, although the distribution with atomic weight has still to be examined. Were the velocities Maxwellian, Lyot's line profiles would indicate a kinetic temperature around 2,300,000°. On the other hand the average level of ionization in Edlén's identifications is 340 electron volts (taking the figures in Swing's Table VIII and weighting the lines according to their intensities), which corresponds to a kinetic temperature 2,600,000°. This is surprisingly close to Lyot's figure. (The term "temperature" is here being used in a very restricted sense, as little more than a description of velocities of atoms or electrons.) It is evident that much light could be thrown on the matter by further study of emission line profiles and of the structure of the Fraunhofer lines appearing in the spectrum of the outer corona. Some of this work can be carried out in full daylight by the Lyot technique; the examination of the Fraunhofer spectrum will need a total solar eclipse.

R. O. REDMAN.

Radcliffe Observatory,
Pretoria:
1943 *November* 27.

* *l'Astronomie*, 15, 211, 1937.

COMETS

During the year 1943 three new comets and three expected periodic comets have been detected. Unfortunately, delays in announcing discoveries sometimes take place owing to the war.

Comet Oterma 3 was discovered by Miss Oterma, at Turku, on April 17. Its magnitude was 15 at the time of discovery.

Comet Daimaca was discovered on September 3 by Daimaca, at Bucharest. It was a diffuse object with a tail when discovered, and its magnitude was 8. It was independently discovered by Peltier.

Comet Comas-Solá 1927 III was found by Miss Oterma on October 2, very close to the position predicted by C. Dinwoodie and W. P. Henderson.*

Comet D'Arrest 1923 II was found by van Biesbroeck on October 24. The elements

Elements of the Orbits of Comets

Comet	T(U.T.)	ω	Ω	i
		° ′ ″	° ′ ″	° ′ ″
Oterma 3 1943 *a*	1942 Sept. 13·65334	359 00 22·7	154 55 55·3	3 58 52·0
Daimaca	1943 Aug. 21·538	36 02 00	82 15 06	161 15 42
Comas-Solá 1927 III	1944 Apr. 11·619	38 51 36	65 43 34	13 43 44
D'Arrest 1923 II	1943 Sept. 18·814	174 18 27	143 54 55	18 01 03
van Gent	1944 Jan. 12·081	33 28 48	61 29 17	142 18 04
Daniel 1909 IV	1943 Nov. 22·189	6 05 02	70 27 14	19 51 11

* *Handbook, B.A.A.*, 1943.

SOLAR ACTIVITY

Sunspots.—The chief features of 1943 were (1) sunspot frequency still falling, (2) spotless days becoming numerous, (3) paradoxically, a marked rise in the number of geomagnetic storms as compared with 1942, (4) the appearance of the first definite high-latitude spot group of the coming 11-year cycle.

The 1943 provisional sunspot number is 16, as compared with 31 for 1942, and 114 and 110 respectively for the maximum years, 1937–38, of the present cycle. The general indications are that the frequency minimum should be reached by the end of 1944. In spite of declining solar activity, there were 9 spot groups, each with a maximum area of not less than 500 millionths of the Sun's hemisphere. Two of the groups exceeded 1000 millionths, their respective dates of central meridian passage being March 10·7 (lat. 5° N.) and April 21·3 (lat. 10° N.).

One of the groups > 500 millionths was in high latitude, 41° south, with C.M.P. on May 13·5. The Mount Wilson polarity observations, indicating that this group is a precursor of the new cycle, are given in *P.A.S.P.*, 55, 182, 1943. At least three other smaller high-latitude spots appeared during the year, viz. June 7–8 (37° S.), July 27–28 (35° S.) and October 9–11 (26° S.). The Mount Wilson observations are awaited to designate another group (max. area 800 millionths: seen from December 17–26 in latitude 22° S.) as an old or new cycle group.

No intense solar flares have been reported in association with any of the spots of 1943. The frequency of fade-outs has also declined, no fade-out being recorded by Cable & Wireless, Ltd., during the last quarter of the year.

COMETS (*continued*)

of its orbit and an ephemeris were computed by F. R. Cripps.* Perihelion passage took place about 3·5 days earlier than the assumed date.

Comet van Gent was discovered on November 27 by van Gent. Its magnitude was 9 at the time of discovery.

Comet Daniel 1909 IV was found by G. F. Kellaway on November 30, its magnitude being 13. It was close to the position predicted by W. P. Henderson and H. Whichello.* Perihelion passage took place a little later than that assumed in the computation.

Comet Daimaca-Peltier-Kellaway, discovered independently by the three observers about the middle of December, was later identified with Comet van Gent. The computation of the orbit presented some difficulties, and fairly large differences in the elements were found by the computers.

Observed during 1943

Comet	q	Period (years)	Equinox	Computers	References
Oterma 3 1943 *a*	3·41564	7·95306	1943·0	1, 2 and 8	*H.A.C.* 662
Daimaca	0·75812	...	1943·0	6, 13 and 15	*I.A.U. Circ.*, 965
Comas-Solá 1927 III	1·7657	8·49620	1950·0	6 and 7	*Handbook, B.A.A.*, 1943
D'Arrest 1923 II	1·3845	6·71086	1950·0	4	*Handbook, B.A.A.*, 1943
van Gent	0·8617	...	1943·0	9, 10, 11, 12 and 14	*I.A.U. Circ.*, 979
Daniel 1909 IV	1·5275	6·79934	1950·0	7 and 16	*Handbook, B.A.A.*, 1943

Computers: 1, Miss McMullin; 2, Miss Oterma; 3, Bobone; 4, Cripps; 5, Cunningham; 6, Dinwoodie; 7, Henderson; 8, Herbig; 9, Jackson; 10, Kahrstedt; 11, Kellaway; 12, Möller; 13, Naur; 14, Sumner; 15, Thomas; 16, Whichello.

M. DAVIDSON.

SOLAR ACTIVITY (*continued*)

Twenty magnetic storms of range $\geqslant \frac{1}{2}°$ in D, or $\geqslant 150\gamma$ in H or in V, were recorded at Abinger, as compared with nine in 1942. With few exceptions, including the only "great" storm of the year on August 30–31, the nature of the disturbances of 1943 was characteristic of those occurring around solar minimum: (*a*) beginning of storms indefinite, (*b*) duration above average, (*c*) tendency to form 27-day sequences, (*d*) a diminished correlation as between storm occurrence and individual sunspots and their associated phenomena in the central zone of the disk. Fourteen of the twenty storms began, however, when the longitude of the Sun's central meridian was between 95° and 215°, indicating a broad "magnetically effective *M*-region" of the Sun.

H. W. NEWTON

Prominences, 1943.—During the year 1943 there has been a marked decline in all forms of solar activity.

The mean daily areas and numbers of calcium prominences as derived from Kodaikanal photographs are given below:

	Areas			*Numbers*		
	North	South	Total	North	South	Total
1943 January to June ...	1·24	1·38	2·62 sq. mins.	4·99	4·85	9·84
July to December ...	1·13	1·43	2·56 „	4·30	4·75	9·05

There has been a decrease of 19 per cent. in areas and a decrease of 18 per cent. in numbers when compared with the previous year's data. Both the areas and numbers show a southern preponderance. The distribution of areas in latitude shows maximum activity between 30° and 35° and between 45° and 50° in the northern and southern hemispheres respectively. The distribution of numbers is maximum between 25° to 40° in the northern and 40° to 45° in the southern hemispheres.

Only 2 metallic prominences were observed during the year as against 26 during 1942. Both were in the northern hemisphere within 14° latitude. Displacements of the *Hα* line in chromosphere and in prominences observed with the spectroscope numbered 47 as against 84 during the previous year. Of these, 19 were towards red and 28 to violet.

One hundred and twenty-two bright reversals of the *Hα* line on the Sun's disk in the neighbourhood of sunspots were observed with the spectroscope, compared with 295 during 1942. The displacements observed in the neighbourhood of sunspots were only 4 as against 9 in the previous year. Of these, 2 were towards red and 2 towards violet. D_3 was observed as a dark line on 94 occasions, compared to 185 during 1942.

The displacements observed in prominences with the spectrohelioscope were 86 as against 210 in the previous year. Of these, 38 were in the northern and 48 in the southern hemispheres respectively, 45 being on the east limb and 41 on the west limb. Displacements to red numbered 50 and those to violet 36. The largest displacement observed was 4·6 A. to red in an eruptive prominence on March 17.

An eruptive prominence photographed on February 18, on the west limb of the Sun, reached a maximum height of nearly 5 minutes.

The mean daily area of prominences projected on the disk as absorption markings (without foreshortening correction) was 1704 millionths of the Sun's visible disk, compared to 2888 millionths in 1942, showing a marked decrease of 41 per cent. The distribution of areas in latitude shows maximum activity between 15° and 20° and between 45° and 50° in the northern and southern hemispheres respectively.

A. L. NARAYAN.

DOUBLE STARS

New Pairs.—Kuiper (*Ap. J.*, **97**, 275), in his fifth note on Proper Motion Stars, gives 6 new binaries with large proper motion. Vyssotsky and Reuyl (*P.A.S.P.*, **54**, 263, 1942) give distant companions to 36 UMaj. An interesting feature of the year has been the discovery of a number of perturbing companions, some of which have unusually small masses (see Orbits).

Measures.—Micrometer measures are given by Schembor (*A.N.*, **271**, Heft 5) and Rabe (*Ver. Mun.*, Bd. 2, No. 1, 1939), photographic measures by Hertzsprung (*B.A.N.*, **9**, 259 and 311, 1942).

Orbits:—

Star	Computer	*P*	*a*	*e*	Publication
		y	″		
ADS 2755	Hertzsprung	103	0·433	0·8	*B.A.N.*, **9**, 258, 1942
,, 4617	Alden	17·5	0·276	0·76	*A.J.*, **50**, 73, 1942
,, 6914	Hirst	214	1·9	0·2	*M.N.A.S. South Africa*, **2**, 100
,, 11005	Hertzsprung	277	1·509	0·7	*B.A.N.*, **9**, 259, 1942
Wolf 358	Alden	3·78	0·043	0·00	*A.J.*, **50**, 139
Cin. 1244	Reuyl	26·5	0·54 a.u.		*Ap. J.*, **97**, 186
		(mass of companion	0·03)		

Perturbations:—

Star	Computer	*P*	*a*	*e*	Publication
		y	″		
ADS 6914	Hirst	35	0·06	0·4	*M.N.A.S. South Africa*, **2**, 100
„ 9413	Strand	2·2	0·020		*P.A.S.P.*, **55**, 28
	(mass of companion		0·1)		
„ 10345	Strand	3·2	0·026	0·4	*P.A.S.P.*, **55**, 26
	(mass of companion		0·6;	see also	*P.A.A.S.*, **10**, 260, 1942)
„ 11046	Reuyl, Holmberg	17	0·015		*Ap. J.*, **97**, 41
	(mass of companion		0·01;	see also	*P.A.A.S.*, **10**, 258, 1942)
			and a note by Russell,		*P.A.S.P.*, **55**, 104)
„ 14636	Strand	4·9	0·020	0·7	*P.A.S.P.*, **55**, 29
	(mass of companion		0·016)		

The perturbation in ADS 6914, though apparently indicated by the available micrometer measures, is not considered as proof of the existence of a third companion; see also *M.N.A.S. South Africa*, **2**, 32 and 111.

Miscellaneous.—Hertzsprung gives a discussion on personal errors in photographic measures of double stars (*B.A.N.*, **9**, 253, 1942), Alden a note on the double star Cor. 193 (*A.J.*, **50**, 139), Thomas a provisional report of an astrometric study of *Algol* (*P.A.A.S.*, **10**, 261, 1942), Holmberg a determination of the mass ratio and parallaxes of *Castor* and 70 Ophi (*A.J.* **50**, 100), Van de Kamp a note on the mass ratio of μ Orio (*A.J.*, **50**, 120) and on the photographic determinations of stellar masses (*P.A.*, **51**, 175), Williams and Vyssotsky discuss the frequency of distant companions (*P.A.S.P.*, **54**, 260, 1942) and Pearce the radial velocity of the visual binary ADS 9744 (*P.A.A.S.*, **10**, 313).

W. H. VAN DEN BOS.

ADDRESS

Delivered by the President, Professor E. A. Milne, on the Award of the Gold Medal to Professor Otto Struve, Director of the Yerkes and the McDonald Observatories

The Council has awarded the Gold Medal of the Society to Professor Otto Struve for his work on the observation and interpretation of the spectra of stars and nebulæ.

It will be my duty and privilege shortly to lay before you the grounds for this award. But before doing so I should like to remind the Society of what an historic occasion this is, historic in the annals of the Society and historic in the annals of the Struve family. For this is the fourth time that a representative of the astronomically minded family of the Struve's has been awarded the Society's Gold Medal—four times in a hundred and eighteen years, or once in each generation.

My predecessors in this office, when presenting Gold Medals to members of the Struve family, have begun by addressing you on the grounds of the award, leaving to the end of their discourses the remarkable family connection of the Struve's with this award. But the present occasion is so notable that I ask to be allowed to dwell on this aspect of the matter first. I should mention that your Council in making this award was totally uninfluenced by the glamour that surrounds the name Struve in the history of astronomy. The present recipient was selected in fair and indeed severe competition with other names, severe not only in these war-time conditions, where the choice of medallist is partly limited by questions of nationality, but severe also by any absolute standard. It is to the glory of our subject that there has never been any shortage of names of the standard of this, the highest award in the Society's gift, and it was partly because of the probable future abundance of astronomers worthy of receiving the Medal that your Council decided to continue the restoration of its normal practice of awarding the Gold Medal at the Annual General Meeting of the Society—a change from earlier war-time procedure fittingly initiated by the award last year to the Astronomer Royal. Professor Otto Struve, I repeat, has earned this distinction in his proper right, by the overwhelming significance and value of his brilliant observational and interpretational work in stellar and nebular spectroscopy. I shall come to this later. First, however, I want to set his contributions in their historical setting, by mentioning briefly the astronomical contributions of his distinguished forbears so far as they concern this Society.

Science in the last century or two has given us many examples of families in which son has followed father with a like distinction. In physics this country has given us the Strutts, the Thomsons and the Braggs. France has produced the Curies, mother and daughter. In meteorology the elder and the younger Bjerknes are famous. Coming nearer to our own subject, the notable and continuously able family of Darwin has given to astronomy a son perpetuated in our George Darwin lectureship, whose son in turn we are happy to number now amongst our Fellows. In astronomy, amongst contemporary names we can claim the Plasketts, and amongst the giants of old the Herschels. But perhaps no family can vie with the Struve family in its devoted pursuit of, and its achieved distinction in, astronomy.

Wilhelm Struve, great-grandfather of our Medallist, was born in 1793 and died in 1864. After working first at Dorpat, he founded in 1839 the Pulkova Observatory, which he directed for nineteen years. He was awarded the Gold Medal of the Royal Astronomical Society in 1826, in the early days of the Society, for his work in discovering and measuring double stars. It was on April 14 of that year—note the date—that J. F. W. Herschel, acting as proxy for Wilhelm Struve, received the Gold Medal at the

hands of Francis Baily, the then President; Gold Medals were also presented in their own right to J. F. W. Herschel and to his collaborator J. South. The work of Wilhelm Struve had been accomplished at Dorpat, with the aid of a telescope made by Fraunhofer. Double stars were then a study of recent date, the subject having been opened up by Sir William Herschel, who, beginning with a knowledge of only four double stars, went on to determine a great many cases of relative orbital motion in close pairs. His work had been continued by his son and by South, working together, and independently by Wilhelm Struve. I have already referred to the Herschels as giants of old, and this bracketing of their names with that of Wilhelm Struve shows that he too is to be regarded as amongst the giants of old. Baily concluded his address of presentation to Wilhelm Struve, even more prophetically than he can have anticipated, by saying: "His services in the cause of science assure us that the name of Struve will be imperishable in the annals of astronomy."

The grandfather of our Medallist was also an Otto Struve. The son of Wilhelm, he was born in 1819 and died on 1905 April 14. He followed his father in the directorship of the Pulkova Observatory, though not immediately; he directed it from 1862 to 1890. Like his father before him, he won our Gold Medal before becoming Director of Pulkova—the year was 1850, during his father's directorship. The grounds of the award were a paper on "The Determination of the Constant of Precession with regard to the Proper Motion of the Solar System". It had been again Sir William Herschel who, in 1783, first drew attention to the proper motion of the solar system towards the constellation Hercules, by considering the directions of the then available proper motions of the stars. The subject was taken up by Otto Struve the elder, who made use of some 265 double stars and 174 wider pairs observed at Dorpat, together with other stars. From their proper motions between 1755 and his own epoch he isolated the geometrical precessional motion. His great problem was how to correct the observed proper motions for the solar motion, which is of course more important for the nearer stars. Struve attempted to connect distance and magnitude, and, adopting $0''.2$ as the annual parallax of a star of the first magnitude, found the Sun's annual motion to be $1\frac{1}{2}$ times the radius of the Earth's orbit. This was a very creditable estimate, the true value being about four times the radius of the Earth's orbit. On this occasion, 1850 February 8, the Gold Medal was presented by Airy.

The third generation in the Struve family to receive the Gold Medal was represented by Hermann Struve, elder son of Otto Struve, grandson of Wilhelm Struve and uncle of our present Medallist. He was born in 1854 and died in 1920. He founded the Berlin-Babelsberg Observatory in 1913. He was awarded the Gold Medal of this Society in 1903 for his work on the satellites of Saturn. This was an arduous and comprehensive performance, involving comparisons of the satellites in pairs made over many years, and a huge labour of arithmetic. He deduced the true position of Saturn's equator, and from the orbits of the satellites themselves went on to determine the changes in their orbits, and so to the masses of the satellites and the shape of the planet. He confirmed Clerk Maxwell's theoretical work on the smallness of the mass of the rings. H. H. Turner, who presented the Medal on this occasion (1903 February 13), described him as "master of all his weapons, from the 30-inch telescope to the complexities of the gravitational analysis". Turner went on: "It is natural that our thoughts should stray to-day from the present occasion to the past; from the man himself to his father, who was awarded our Medal half a century ago; and to *his* father, who received it a quarter of a century before that. For the third time we welcome the name Struve to our list of honour, and a welcome as cordial awaits an unlimited number of those who in the future may produce work of the same quality as their forefathers." "We are glad to think that its value to him will be enhanced by the knowledge that it ranks him with his venerable father" (then still living at the age of eighty-four) "and his famous grandfather."

That welcome then hypothetically held out by Turner we extend in actual fact to-day

to the fourth generation, to the great-grandson of Wilhelm Struve, to Otto Struve the second. He has been faithful to his family tradition, and is ranked with his progenitors. *Our* Otto, as I may now call him, is the son of Ludwig Struve, who was the younger brother of Hermann Struve and the second son of Otto Struve the first. Ludwig Struve, it should be recalled, was also an astronomer of reputation: he was Professor of Astronomy in the University of Kharkov, and died in 1920. Thus do we welcome, in 1944, the successor to honours previously bestowed by this Society in 1826, 1850 and 1903.

Double stars, the solar motion and precession, the Saturnian system—after these three major and representative astronomical themes it is meet and right that the youngest of the Struves should have devoted himself to astrophysics, that younger sister of the classical astronomy. And with no sparing hand; for he has distinguished himself equally in the precision and wealth of his spectroscopic observations, his fertility in the design of new spectrographs, his power of execution of new astrophysical projects, his modernity in his analysis of spectroscopic phenomena, the width and generality of the problems he has selected and attacked, and the brilliance of his solutions of these problems and the bold sweep of his interpretational grasp. Add to this that he has throughout shared in the actual observing and reduction of spectrograms; that he has edited the senior journal in astrophysics and personally reviewed most of recent astrophysical literature in its pages; that he has followed the family tradition in founding a new observatory, but that he has exceeded the family tradition by directing not merely one but simultaneously two great observatories; and lastly that he has inspired and gathered round himself a devoted band of loyal and original-minded co-workers—and we have the parts of an astrophysicist indeed, one who has carried the family fame, in a new subject, to a new continent, and made good.

I would now remind you that the Gold Medal is awarded for his individual work, some of which I will try to pass in review. To deal with the whole of it would be beyond my time and my powers. His first paper, in the *Astrophysical Journal*, is dated 1923. Appropriately enough, it relates to observations of a double star—the spirit of his great-grandfather was stirring in him. These observations, like many of those which followed, were made with the Bruce Spectrograph of the Yerkes Observatory. The star was 9 Argus, a star which had been first marked for observation at Yerkes in 1905 by E. B. Frost, under whom Struve was working. This is a visual double (of period $23\frac{1}{3}$ years), which can also be observed spectroscopically. Combination of visual with spectroscopic elements gave him the mass-ratio as 0·6, and the total mass of the system as 1·0–1·1 $\odot$. This paper was followed in 1924 by observations of the spectroscopic binary 43 θ^2 Orionis, of which he took 39 plates. This star showed a K line of Ca^+ not participating in the periodic shifts of the other lines—an observation foreshadowing studies to come; it was found that this K line, and the Balmer lines in emission, gave a radial velocity nearly the same as that of the surrounding Orion nebula, and differing by 20 km./sec. from that of the star proper. Spectroscopic binaries as a whole now engaged our Medallist's attention. He examined the data for 144 such binaries and found a relation between their periods P and semi-amplitude of velocity-variation K in agreement with their supposed binary nature for periods in excess of 2·4 days, whilst for those which were Cepheids he found no variation of K with P; as for the shorter-period stars, he found them to form two groups, true binaries and Cepheids. The most interesting general conclusion emerging from the study was that the average total mass of the actual doubles was 3$\odot$. The next year or two saw more work on particular spectroscopic binaries, ending with a very substantial paper in 1926 (in conjunction with Frost and Barrett) giving the radial velocities of 368 B-type stars, determined from Bruce plates taken in the years 1901–25. Of these, 43 per cent. were found to be spectroscopic binaries. There resulted a determination of the apex of the solar motion as $\alpha = 283°{\cdot}8$, $\delta = +11°{\cdot}7$, velocity 17·2 km./sec. and a K term of 5·6 km./sec. The p.e. of an average radial velocity was $\pm 3{\cdot}5$ km./sec.; from

a single spectrogram it was ±9·0 km./sec.; and the average residual radial velocity was 10 km./sec. This monumental paper gave the observational data both for each star, averaged, and for each separate plate of each star.

I have dwelt at length on these early studies by our Medallist because, apart from a natural transfer of interest from visual binaries to spectroscopic binaries, it looked as though the younger Otto were merely following lines of attack opened up by his grandfather and great-grandfather. Our Medallist was to turn again and again to spectroscopic binaries in the years that were to follow, especially to such difficult stars as 27 Canis Majoris, with its enormous inferred mass, and 29 Canis Majoris. But wider interests lay ahead.

It had been in 1924 that J. S. Plaskett had published his famous investigations on interstellar calcium lines. I have already reminded you that these lines do not share in the oscillatory motions of those in the spectroscopic binaries they accompany, and indeed have their own radial velocities in single stars. The exceptional behaviour of these lines had been discovered by Hartmann in 1905, in the star δ Orionis, but it was J. S. Plaskett who cleared the matter up, showing that the K-line velocities were the reflex of the solar motion, and that accordingly the Ca^+ cloud was associated not with particular stars but with the galaxy as a whole. Further, Eddington had made extensive theoretical calculations on the same subject in his Bakerian Lecture for 1926. To this then rapidly developing subject our Medallist in 1926–27 turned his attention, using for the purpose a collection of slit-spectrograms taken at the Mt. Wilson, Victoria, Lick and Yerkes Observatories, together with additional plates of his own taking secured with the 60-inch reflector at Mt. Wilson. He found that interstellar K lines showed their greatest intensity for stars at a distance of 500–600 parsecs, this being based on intensity estimates in 321 stars. He established the important point that in *visual* doubles the two components gave identical intensities for the K line. He even found one star, 2 Lacertæ, which showed an interstellar K line *between* two "stellar" components. He made a special study of the Ca^+ cloud for 36 stars in Cepheus, finding the K lines systematically more intense than in Orion. The distance of the Ca^+ cloud in Cepheus he put as beginning at 250 parsecs away, whilst the dark nebula in the same region, indicated by star counts, is at 350 parsecs. Interstellar lines were not found in types later than B3—why, seemed difficult of explanation, but was perhaps due to the failure of later-type stars to contribute free ions under radiation pressure. The Ca^+ cloud, as disclosed by Struve's investigations at this stage, seemed to correspond to Charlier's local cluster of B-type stars. Struve found, as predicted generally by Eddington, that the interstellar lines nearly doubled in strength for a doubling of the distance of the star concerned. Though the exact significance of this, quantitatively speaking, depended on the calibration of Struve's intensity scale, it was clear that the interstellar cloud was widespread.

He returned to the subject in 1928 with a study of 1728 stars of types O to B3, and 338 later-type stars. This larger amount of material was made available by the use of Harvard objective-prism spectrograms. The general conclusion was to confirm that the intensity of the interstellar K line was a function of distance, for each spectral type; the distances were obtained by the assumption of a correlation of absolute magnitude with spectral type. Spectroscopic binaries with *oscillating* K lines were shown to be capable of interpretation in terms of blends. The existence of peculiar radial velocities in the interstellar K lines suggested the occurrence of characteristic velocities in different parts of the Ca^+ cloud.

The fruits of these observational studies were seen in a piece of successful collaboration between our Medallist and B. P. Gerasimovič, in an important paper in 1928 entitled "The Physical Properties of a Gaseous Substratum in the Galaxy". Whilst Eddington had started on the basis of an Emden isothermal gas-sphere, Struve and Gerasimovič started from the observational end. By first determining the absorption coefficient of the Ca^+ cloud per cm., and then using the absolute value of the atomic absorption

coefficient as disclosed by my own chromospheric studies, combined with line-width estimates, they estimated the mass-density of Ca^+ gas in the galaxy as about $3{\cdot}6 \times 10^{-32}$ gr./cm.^{3}. Allowing for its state of ionization, which, owing to the high temperature of even dilute starlight (as shown by Eddington), is mostly the doubly ionized state, and allowing further for the probable cosmic abundance of calcium relative to the other elements, they deduced finally the density of the gaseous cloud forming the substratum as of the order of 10^{-26} gr./cm.^{3}. The total mass of the cloud they estimated as not greater than one per cent. of the total stellar mass per unit of volume. The rotation of the galaxy had then recently been discovered by Oort, and Gerasimovič and Struve were able to confirm J. S. Plaskett's result, that the Ca^+ cloud shared the rotation of the galaxy. It was of course subsequently shown by Plaskett and Pearce that the degree of rotation corresponded to a mean distance one-half that of the stars concerned, thus suggesting a generally uniform distribution of the cloud.

In 1929 our Medallist went on to use the Stark effect in line spectra to determine the absolute magnitudes of certain B-type stars. How this was possible can be seen as follows. He first showed that intermolecular electric fields could cause the appearance of "forbidden" lines in stellar spectra, and by means of various criteria deduced that the Stark effect is more pronounced in dwarfs than in giants. This is in accordance with our expectation that dwarfs, having larger values of surface gravity, will have higher pressures in their atmospheres. Struve's Stark-effect investigations confirmed the usual estimate of 10^{-4} atmospheres pressure, as deduced from Saha's theory of high-temperature ionization. Now the value of surface gravity can be expressed in terms of absolute magnitude and effective temperature, and hence for stars of given colour-type or spectral class, observations of the relative strengths of lines influenced by the Stark effect—that is, by the degree of packing of neighbouring ions—can be used to derive absolute magnitudes, just as in Adams and Joy's original method of spectroscopic parallaxes. In this way Struve used the ratio of the forbidden line of helium λ 4470 to the permitted line λ 4472, and deduced for example that the star 67 Ophiuchi is highly luminous, of an absolute magnitude 3·0 magnitudes brighter than the star 88 γ Pegasi; and he estimated its distance as 600 parsecs.

In collaboration with Shajn, Struve had communicated to our own *Monthly Notices* in 1929 a paper on the contours of absorption lines in rapidly rotating stars, a subject which had been independently investigated by J. A. Carroll in 1928. Carroll's work, which was of great beauty, was mathematical. Struve and Shajn adopted a more empirical method of approach. They pointed out that the profile of an absorption line so produced would be "dish-shaped" if the axis of rotation were approximately normal to the line of sight; that is to say, the lines should be shallow, but with well-defined edges. Struve and Shajn succeeded in correlating line-widths with rotational velocities in short-period spectroscopic binaries, on the assumption that in such close binaries the rotational velocities of the components must be approximately the same as the orbital angular velocity; and they suggested that the results indicated rotational equatorial speeds of the order of 20–150 km./sec., with most of them of the order of 50–70 km./sec. In 1931 Struve published a study of bright lines in stars of type B in which he showed that stars having widely separated, doubled bright lines are characterized by extremely flat and broad absorption lines, suggestive of rapid axial rotation, about an axis nearly perpendicular to the line of sight, of the order of several hundred km./sec. He suggested further that such stars, being near the limit for stability, would be ejecting matter at their equators, and this matter would form a nebulous ring and so explain the accompanying doubled emission lines. Struve was of opinion that "the theory of ionization accounts so satisfactorily for even small changes in the intensities of stellar absorption lines that the occurrence or non-occurrence of bright lines must be attributed to real structural differences amongst the stars, and not to changes in the conditions of excitation. In analogy with the Novæ and with the Wolf-Rayet stars, it is natural to attribute the bright

lines to an outer gaseous envelope or nebula. But in Novæ the nebulous material is receding from the star. . . . The much greater stability of the phenomena observed in Be stars and especially the absence of absorption on the violet sides of the emission lines suggest that the nebula in a Be star is a permanent feature." He claimed that only those stars showing dish-shaped absorption contours, *i.e.* exhibiting rotation effects, tended to possess bright lines; further, narrow emission lines were associated with narrow absorption lines, the two features together indicating a star whose axis tended to be more nearly coincident with the line of sight. In 1930 he had shown that very rapid rotation is peculiar to the earlier spectral classes, a conclusion which was confirmed by another study of the contours of lines in the components of spectroscopic binaries. For example, in α Virginis, the two components were found to give quite different line-profiles, corresponding to equatorial velocities of 200 km./sec. and 50 km./sec. respectively. The single star η Ursæ Majoris was also studied and found to be similar to the fast-spinning component of *Spica*.

Our Medallist's output in these years, and indeed in all years since, was and has been enormous. He followed up his studies of axial rotation, in 1931, with a comprehensive standard list of wave-lengths in the spectra of B-type stars together with intensity-estimates, and applied it to elucidate many points in the theory of high-temperature ionization, studying lines of helium (neutral and ionized), and ionized oxygen, nitrogen and silicon.

In 1934 Struve returned to the subject of the calcium clouds. By comparing distances determined from line-intensities with distances determined by means of the galactic rotation effect, he inferred that condensations must be present at different places in the cloud. He was able to use his results to deduce the distance of the galactic centre as 10,000 parsecs.

E. B. Frost, the Director of the Yerkes Observatory, had retired on 1932 July 1, and Struve, who had worked under him for ten years, then became Director. It is apparent now that he immediately set about inaugurating stellar and nebular spectroscopic programmes of the widest interest, and that from this time forth his was the driving force behind the brilliant output of the staff of the Yerkes Observatory. As was natural, joint papers with members of his staff now become more frequent. But it is usually easy to recognize the characteristic Struve touch, the touch of one to whom the individual stars, with their individual peculiarities, were personal friends. And ever and anon Struve would himself summarize and discuss the recent progress in each new subject under development, and illumine it with statesmanlike breadth of vision, sure judgment and acute deductive power. Throughout he saw each problem *physically*. And though he attracted round himself a brilliant band of ardent mathematicians from all parts of the world, who were able to develop his physical ideas in mathematical symbolism, the inspiration and initial originality were usually Struve's, as they would be the first to admit. One gets the impression, from the pages of the *Astrophysical Journal*, of a steadily maturing mind, no longer content with defeatist solutions of astrophysical problems but bent on winning through to the essential physical explanations. As the decade of his Directorship wore on, there is a veritable crescendo of coruscating papers from the Yerkes Observatory, to which from 1939 the McDonald Observatory added its share.

For example, in 1933, in a broadly conceived paper, Struve dealt with the problem of the need for a more detailed classification of stellar spectra than Miss Cannon's linear sequence. It is well known that this sequence corresponds in the main to an "effective temperature" classification, with surface gravity (or absolute magnitude) as a cross-parameter in each division. Struve questioned whether this classification was adequate, or whether (apart from rotational effects) other parameters might not be needed. He concluded that to make possible an advance in our knowledge it was imperative "that new and more refined methods than are at present available be used in stellar spectroscopy". He suggested that the principal criterion for the B stars should be the intensity

of *He*I 4472; the varying intensities of *Mg*II 4481 would then show how other parameters were at work. To separate more parameters, more lines would be needed.

In 1935 he re-examined 27 Canis Majoris and decided that on the evidence of his new observations it ought to be dropped from the list of spectroscopic binaries, owing to the non-recurrent character of its velocity curve.

In the same year, with Morgan and Henyey, he made a special study of the spectrum of P Cygni, the forerunner of a multitude of detailed studies, especially later with P. Swings, of stars with peculiar spectra. For P Cygni 158 absorption lines were measured, of which all but 4 were identified, and 63 emission lines, of which all but 15 were identified. The absorption lines were found to be characteristic of a higher level of excitation than the emission lines, as measured by the ionization potentials. The general results suggested a shell of gas, with an accelerated outward motion. P Cygni, it may be mentioned, is an old Nova (1600). The central star has an effective temperature of 20,000°, and is about 1000 parsecs from the Sun. Selective interstellar absorption (the interstellar K line is exceptionally strong) reduces the effective temperature to approximately 6000°. The spectrum observed originates in an expanding nebular shell, its velocity varying from 50 km./sec. to 200 km./sec.

From 1936 onwards, the brightnesses of nebulæ, both dark and bright, began to engage his attention. With Elvey, he attempted to answer such questions as: What are the surface brightnesses of dark nebulæ? These investigators constructed a Fabry photometer for use in the focus of the Yerkes 40-inch refractor; the method was photographic, and the measures made finally with a Hartmann microphotometer. Their very accurate measures disclosed an unexpectedly small value for the excess of surface brightness of the dark nebulæ over that of the comparison regions, and they concluded that the albedo per nebular particle was smaller than had been surmised, and that scattering in interstellar space is nearly as efficient as scattering within dark nebulæ.

This negative result was followed by one of emphatically positive a character. It had been shown by V. M. Slipher, Hertzsprung, Hubble and Zanstra that galactic nebulæ derive their illuminating power by simple scattering of the light of near-by stars. Struve and Miss Story went on to investigate the albedos of the particles forming diffuse nebulæ; in the region of ρ Ophiuchi it came out as 0·7. Next, Struve, Elvey and Roach determined the colour index of the nebula in Scorpion illuminated by *Antares*, which you will remember is a red giant (*c*Mo). By the use of a Schmidt camera they compared photographs taken with photovisual and photographic emulsions, and they found for the colour index of the nebula the enormous positive value of 1·9 mag. The shape of the nebula was brilliantly outlined on photovisual plates, whilst practically invisible on photographic plates. Nebulæ illuminated by B stars, on the other hand, were found to have the negative colour index of −0·4 mag. This work was of the utmost value in establishing with certainty the existence of reflexion nebulæ. The colour index of the night sky, in the same investigation, was found to be +0·5 mag.

A further paper was devoted to the dark cores and bright rims of many of Barnard's dark nebulæ. The narrow bright rims found in emission nebulæ but not in nebulæ having continuous spectra were investigated on the basis of the Lommel-Seeliger theory.

For further studies on nebulæ, Struve, van Biesbroeck and Elvey constructed a 150-foot nebular spectrograph at the McDonald Observatory on Mt. Locke, and described it in 1938. It was being used by Struve in 1939 to estimate the density of excited hydrogen atoms per cm.^{2} in the line of sight for certain regions of the Milky Way which were the seat of Balmer emission. Later, with the new quartz spectrograph of the McDonald Observatory Struve and Swings observed the ultra-violet region $\lambda\lambda$ 3200–3900 in the spectra of various early-type stars; they verified the presence of expanding shells, and discovered some extremely "hot" nuclear stars. The stars studied included Oe stars, P Cygni stars, Be stars, and spectra showing simultaneously an M-type spectrum and forbidden lines of high excitation. Amongst them was Z Andromedæ, previously studied

by H. H. Plaskett; this had recently shown a remarkable change from an emission spectrum to one of P Cygni type.

As a consequence of these studies of peculiar spectra, Struve and his collaborators were able to infer, from the effects of dilution of radiation, the ratio of radius of parent star to radius of expanding shell. This led in turn to the suggestion that such expanding shells tend to be associated with early-type binary stars on the verge of instability. As an example of his powers of interpretation, I may instance his re-attack, in collaboration with Kuiper, on the problem of the structure of β Lyræ, that obstinately mysterious eclipsing binary. Their conclusions are that the system consists of a central star of type B9, with a small F-type companion which eclipses it, the whole surrounded by an expanding or spirally moving shell of gas and the space between the two stars being the seat of currents of gas, to and from the primary, arising from the instability of the primary. These general physical ideas have been given mathematical expression by Kuiper.

As recently as 1942 Struve followed up his numerous, indescribably detailed studies of stars with peculiar spectra by a markedly individual discussion of the variety of problems they give rise to. He refined the simple notion of a reversing layer, or an extended stellar atmosphere, by showing from the available observational data that in an ideal case three distinct layers co-exist: (1) the ordinary reversing layer; (2) a stationary, high-level, chromospheric layer producing a fine-line spectrum of α Cygni type; (3) above this an expanding layer producing a spectrum of P Cygni type.

It is scarcely possible to do justice in present circumstances to the most recent of Struve's contributions, for discussion of them in astronomical circles in this country has scarcely been possible since the outbreak of war. I will only say that they have continued unabated in quality and quantity.

I said at the outset that the individual work of Otto Struve covered astrophysics in the widest sense. The calcium cloud, the Stark effect, stellar rotations, the properties of the B stars, expanding stellar shells, peculiar spectral types, reflection properties of nebulæ—to each of these he has made both pioneering and sustained contributions of permanent value. He has shown personal skill in his choice, design and use of instruments, and scientific imagination of the highest kind in his interpretation of the results of their use. He has well earned our Gold Medal.

But this is not all that I would bring to your notice. I have already mentioned repeatedly the Yerkes and McDonald Observatories. But I have not paused to relate how Struve, whilst full-time Director of the Yerkes Observatory and Professor in the University of Chicago, found time to plan, construct and inaugurate a new observatory in Texas, and thereafter, becoming its Director whilst not ceasing his Yerkes work, to execute a brilliant programme of observations at the newly founded McDonald Observatory on Mt. Locke.

Mr. William Johnson McDonald, of Paris, Texas, bequeathed a large sum of money to the Regents of the University of Texas "to aid in erecting and equipping an astronomical observatory as a part of the University for study and the promotion of the study of astronomical science". The result was that by an almost unprecedented piece of University collaboration, under Struve's *de facto* leadership, the Universities of Chicago and Texas agreed to co-operate in constructing and maintaining such an observatory. The Director of Yerkes was to be the Director of McDonald, and the latter's staff were to be supported by the University of Chicago. A site was selected by Struve in the Davis Mountains in Texas, and an 82-inch reflecting telescope was constructed. It was declared open for research on 1939 May 5—your present President had the honour of being present at the dedicatory exercises. The contract had been given to the Warner and Swasey Company in 1933, the observatory building was completed in 1936 and the great telescope in 1939.

Each of our previous Struve Gold Medallists has either founded or directed with great distinction a great observatory. Our present Medallist has founded one new observatory, already great, and he is directing two. It is generally recognized that the

speedy and successful carrying out of the project of the joint observatory, up to the final installation of the 82-inch telescope, and the outlining and inauguration of its research programme, are due to the scientific and executive abilities and marked personal qualities of Dr. Otto Struve, our Medallist of to-day.

With the Gold Medal the Society tenders to Dr. Otto Struve, Director of the Yerkes and McDonald Observatories, its heartfelt congratulations on the outstanding success and comprehensiveness of his personal contributions to the science of astrophysics, together with its profound wishes for the welfare in the future of the observatories he directs and of the devoted staff associated with him and with them.

ADDRESS

Delivered by the President, Professor E. A. Milne, On the Nature of Universal Gravitation

I quote the following passage from Grant's *History of Physical Astronomy*:—

> Among the various circumstances which are calculated to excite our admiration, while reviewing this portion of the history of Physical Astronomy, not the least remarkable are the resources of the transcendental analysis; by means of which the geometer has been enabled to unravel the most complicated relations of the system of the world, and to decipher in the anomalous movements of the celestial bodies the constant operation of one all-pervading principle. In vain would the human mind have ever attempted to penetrate into the more recondite parts of the theory of gravitation without the aid of this powerful instrument of research. Its assiduous cultivation was, therefore, essentially necessary for the development of that theory; and on this account the pure analysts, such as Leibnitz and the two eldest Bernoullis, deserve to be associated with those who have more directly contributed to the progress of the science. "The discovery of the system of the world by Newton", says Delambre, "was a fortunate event for geometers. Never could the transcendental analysts find a worthier or more sublime theme. Whatever progress is made in it, the original discoverer will always maintain his rank. Lagrange, who often asserted Newton to be the greatest genius that ever existed, used to remark also—'and the most fortunate; we do not find more than once a system of the world to establish.'"

By the phrase, "the system of the world", as used by the eighteenth- and early nineteenth-century mathematicians, was evidently meant the solar system. There was, or was then, only one solar system to be discovered, and that discovery, in its dynamical relations, was made by Newton. Thus Newton is acclaimed by Lagrange as fortunate in his generation.

But "the system of the world" means to-day something vaster, immensely vaster —I will not yet say infinitely vaster—than the solar system. It means something vaster even than the galactic system. It means the totality of all galactic systems. If Newton was fortunate in his generation, how much more fortunate is every astronomer of to-day, born to live at a time when the discovery has been made of a system, if system we can yet call it, grander in every sense—grander in its structure and grander in its constituents —than the solar system. We must all consider ourselves fortunate to have the opportunity of sharing in the analysis of this grand "system of the world".

The vista of research thus opened is so splendid as to be dazzling. We have to re-orient our terminology even to speak of it. The phrase "solar system" is itself a

relic of local patriotism; we now have to speak generally of "planetary systems". Similarly we can no longer confine the term "galactic system" to our own galaxy, or inflate our own egoism by speaking of "extra-galactic" systems. I shall speak of these systems simply as "galaxies".

The system of the galaxies, then, calls for dynamical examination. If regarded as a system of particles, will their motions prove to be executed under gravitational attractions of a like kind with those that rule in the solar system? Or will the system of the galaxies, the universe as a whole, call into existence another Newton, discoverer-to-be of a new world-force?

The system of the galaxies, as disclosed by the great American telescopes, is as different in character from our own solar system as the wildest imagination could have pictured. Instead of a massive, dominant, central body, it has no central body. Instead of consisting of a comparatively small number of objects, it consists of literally unnumbered members, each a galaxy. Instead of motions of revolution in approximately closed orbits, the only motions we yet know of are motions of mutual recession. Instead of a well-mixed distribution of velocities as in a gas, there is velocity-segregation. Instead of the stability and quasi-permanence of the solar system, the system of the galaxies exhibits a steady expansion, and, moreover, unlike the solar system, in which the more distant bodies move the more slowly, here the more distant bodies move the more rapidly—relative to ourselves. Is it possible for so strange a system, so dissimilar to the solar system, to be obedient to the same law of gravitation which operates in the solar system? This is the great challenge offered by the universe to the astronomers of to-day.

The Presidential Address is usually delivered from beneath a portrait of Newton. And as in a sense his genius presides over all our meetings, it may savour of *lèse-majesté* to offer as a subject that which he made his own. But surely Newton would not have us be cowed, by the supremacy of his genius, into leaving undiscussed the great problem which in a sense he has bequeathed to our care. Now that another "system of the world" has presented itself, it becomes our duty and our business not to be overwhelmed by the problem of its constitution but to grapple with it.

In this address I propose to trace briefly the influence of the discovery of the expanding universe on recent gravitational theory, and then to summarize the results of some of my own work on the subject.

You may say that a theory of gravitation has already been developed embracing and indeed transcending Newton's, namely Einstein's so-called "general" theory of relativity. Wonderful as this theory is in many ways, we must not let ourselves be blinded to its shortcomings. It is a good rule that however well a theory appears to fit the facts, it cannot be accepted as satisfactory if it is not *philosophically* satisfactory. Einstein's theory does not in any way explain or account for gravitation. It is a description of gravitation, so far as it goes, which transfers the burden of accounting for the motions of bodies in one another's presence from the particles themselves to the space surrounding them, or rather to the space-time surrounding their world-lines. It ascribes their motions to the curvature of this space-time. In doing so it in effect again admits the notion of an æther—a space-time-filling medium—which the previous march of science had done so much to abolish. It seems to me to be not in the true line of descent of mathematical physics: it abandons the space customarily employed in physics, it abandons the Lorentz formulæ which had been so easily assimilated in the metabolism of physics; and, compared with the Lorentz formulæ, it has had singularly little influence on the development of mathematical physics proper. It has not readily accommodated itself to the description of electromagnetic phenomena. It is in fact a cul-de-sac, a fine but barren flower. It has other shortcomings: it does not account for the genesis, or sign or magnitude of the so-called constant of gravitation γ; and its whole use of space-time is blind to the fundamental inescapable fact of the one-way passage of time,

the uniqueness of direction of our sense of time passing, the difference between past and future. This difference is within the judgment of each individual observer for events "at himself"; he cannot deny it, and no theory of so fundamental a thing as gravitation can itself claim to be fundamental which does not take into account the fact not only of time passing but of time passing in a determinate sense. Moreover, Einstein's theory is essentially a macroscopic, not a microscopic, theory: its field equations are expressed in terms of a density ρ which is a smoothed-out average. If, as seems probable, the ultimate entities in nature are singularities, a theory which deals only in gross averages cannot hope to contribute to our microscopic knowledge of nature. For example, its claim to establish the conservation of linear momentum by means of its field identities is not fully justified, for the proof is inapplicable to an assembly of singularities. I have, in as yet unpublished work, obtained from my own theory of gravitation an identity of a different kind, applicable to singularities, Lorentz-invariant, and implying as I believe the required generalisation of the principle of linear momentum. I merely mention this here to illustrate my contention that our horizons should not be bounded by Einstein's theory of gravitation.

I think that these considerations should weigh in the balance against the supposed fulfilment of the "tests" of Einstein's theory. The theory implies certain consequences; but the consequences do not imply the theory. Moreover, men of science have probably been too easily satisfied that these "tests" have been obeyed observationally. Though I speak on this point with hesitation, conversations with acknowledged authorities, more especially at the former Einsteinturm at Potsdam, have distinctly shaken my confidence in the numerical agreements in all three "tests". I do not think it is necessarily a sign of a suspicious character to assert that we should try to ascertain the facts rather than to test Einstein's theory, and whilst this theory *may* prove to be a sufficiently accurate description of *contemporary* gravitation, I think it not impious to question a theory which is philosophically so unsatisfying. I am, moreover, convinced that over long periods of time it is not true.

Einstein's theory arose as a generalisation of classical mechanics and gravitation; it did not arise from the problem of the structure of the expanding universe. But it was not slow to be influenced by cosmological considerations. Einstein was quick to point out that a *static* universe, homogeneous and obeying Newtonian mechanics and gravitation, in Newtonian flat space and employing Newtonian time as its fundamental independent variable, could not be satisfactorily constructed. This was before the phenomenon of the apparent mutual recession of all galaxies was fully evident. Now there were three ways of escape from this apparent impasse: one was to modify the law of gravitation, a second was to abandon infinite space, a third was to abandon equilibrium and introduce motion. Einstein had already formulated his law of gravitation in the light of his theory, both in empty space and in matter of mean density ρ. He now chose to modify this law still further, by introducing a purely hypothetical new constant λ, the so-called "cosmical constant", a constant of a very unconvincing character; unconvincing because it corresponded to making distant massive particles repel one another with a force increasing as the distance, in addition to their inverse square attraction. Thus the theory now incorporated *two* physical constants whilst providing no prior explanation of the existence of either. Einstein now saw that the difficulties associated with infinite space on Newtonian lines could be removed by the hypothesis of *spherical* space. (Actually the idea of spherical space occurred to him first, and the introduction of λ was consequential, but I have stated a more logical sequence.) The universe was now supposed to possess a positive space-curvature, and to be of finite mass.

Various analogies have been suggested to give a simple physical picture to this "curvature of space", but I do not think that any of them are convincing, or free from objection. I do not question that spherical space is a concept logically self-consistent

and free from internal contradiction, any more than a four-dimensional cube is a concept free from internal self-contradiction. Only, a four-dimensional cube cannot be constructed in nature. The point is not whether a spherical universe is free from internal self-contradiction, but whether it can exist. The analogy with the experiences of two-dimensional inhabitants on the surface of a sphere is imperfect because it relies for its effect on our own acquaintance with a third dimension. It would be quite easy to convince such beings of the "real existence" of a *third* dimension. But it would be impossible to convince physicists of the real existence of a *fourth* dimension. The limitation of the number of dimensions realised in nature to *three* is an essential fact about nature, an empirical fact like the passage of one-dimensional time; and though space of positive curvature does not *logically* require more than three spatial dimensions, we cannot formulate a rational picture of it without recourse to a fourth dimension. The objects in an Einstein spherical universe cannot be put into correspondence with objects in nature unless every object at a distance is theoretically visible in two opposite directions, the one showing its front, the other its rear. I decline to accept this fantasy until it is known that there is no other way out. Fortunately there is.

This question of the possibility of the existence of a spherical-space universe is often quite unjustifiably mixed up with the question of the *hugeness* of the universe. It is said that the curvature necessary is so small that it is undetectable in ordinary experience. But in the analogy of the two-dimensional inhabitants of the surface of a sphere, this ultimate deduction that a third dimension "really" existed would be justified whatever the curvature of their world; it would be no excuse for their inability to discover the third dimension to plead that the curvature of their world was "very small", for the direction of the third dimension would be quite independent of the size of the curvature. In the same way, if a spherical universe represented our world, we ought to be able to point out the direction of a fourth dimension, in spite of the smallness of the curvature. Many sins have been laid on the broad back of the universe, owing to its size. But this apology for the non-discovery of a fourth dimension as resting on the smallness of curvature is without justification.

But I have occupied you too long on this topic. Let me say that I believe that a spherical space of positive curvature is incapable of being realised in nature. Space of negative curvature does not involve similar difficulties, and it naturally arose in my own investigations.

Although Einstein's static solution thus involved difficulties of a twofold character, the followers of Einstein soon proceeded to the third possibility, and incorporated into the solution the recently discovered attribute of *motion*. The pioneer steps were taken by Friedmann, Lemaître and Einstein and de Sitter. These various writers, in papers of great technical distinction, introduced the idea of a metric or space-time curvature depending on the time-co-ordinate of the event in question. The galaxies were represented as particles of fixed spatial co-ordinates, swept apart by the natural expansion (or contraction) of the section of space-time by a surface "time-co-ordinate = constant". These theories are accordingly known as "expanding space" theories. They all gave velocity-distance relationships of the type found by Hubble—velocity proportional to distance from the observer. They were all founded on space-times of *constant* curvature. And since it is the density of matter which controls curvature in general relativity, the physical systems they represented were all *homogeneous* in the sense that the density at all points of a section made by a certain surface "time-co-ordinate = constant" was itself constant. This homogeneity was to be understood in a technical sense, because the simultaneity-surface over which the density is constant—a three-dimensional volume—did not correspond to simultaneity in the observer's sense. But it is important to realise that this homogeneity was assumed outright, and not a deduction from the theory. Made by those workers, in those days, the assumption passed without challenge; but when I came to make a similar assumption later, and showed that this assumption alone

was sufficient to determine both the nature of the expansion and the law of gravitation without the apparatus of space-time and field equations, there was a great outcry. For the moment we note that to deal with this great question of the expanding universe, "general" relativity made two sets of assumptions: (1) the set of assumptions involved in the theory itself, namely the connection of gravitation with metric, and motion along geodesics; (2) an assumption about the universe itself, namely homogeneity.

But clouds soon began to appear on the horizons of these theories. Einstein had begun with a static spherical universe (of positive curvature), with the aid of a cosmical constant λ. It was natural, when motion was introduced, to retain both curvature and λ. But it soon became apparent that the former was arbitrary and the latter not necessary. The curvature could be taken, in these expanding universes, as positive or negative; or even zero. In a famous paper by Einstein and de Sitter, the space of the universe once more returned to unending, flat space, the space of Newton but with a time-coefficient in the metric! It was next noticed that there was no longer any need for the "cosmical constant", questionable as this had always been. Again, strange as had been the translation of the static spherical world into a world-picture, this was as nothing to the incongruities and absurdities which were implied by the translation of these expanding metrics into a world-picture. They implied the continual "swimming into our ken" of newly created galaxies at great distances; creation of matter was implied to be continuously going on, at the present epoch; and, what was more, could be in principle *observed* to be going on. Lastly, as the investigations developed, it was found that the expanding universe was not the only solution of the equations, but that contracting and even oscillating universes were equally implied. No decisive criterion could ever be found, on these theories, which determined whether our actual universe should be expanding, oscillating or contracting; whether its curvature should be positive, zero or negative; or whether the cosmical constant λ should be positive, zero (in which case it would not exist) or negative. It was small wonder that general relativity was unequal to predicting the direction of the motion, for at its outset, as we have seen, it failed to distinguish between past and future: it was blind to the one-way passage of time. It is true, indeed, that certain attempts were made to show that an Einstein static universe was unstable, and would necessarily generate expansion. Waiving the serious difficulty of asking how *space* could be unstable, or what was meant by the instability of space, as I view it the instability envisaged was akin to that of a rigid pendulum balanced *above* its point of support; it tends to depart further from its former state of equilibrium, and so undergoes a displacement paralleling expansion. But it subsequently reaches a maximum displacement and then climbs asymptotically towards its original configuration, paralleling the subsequent *contraction* of some of the model expanding universes. These phases of contraction proved on analysis to imply the annihilation of matter—its disappearance from observation—at the corresponding places where it had been created during the phase of expansion. Thus instability of a static configuration lent no assistance.

I have learned during this war, in my career as temporary civil servant, the value of general appraisals of situations made by people presented with, but not concerned with, the technical details. My appraisal of the situation in the early 1930's was that the state of chaos I have described, to which the theory of relativity had led, had brought general discredit on cosmology in the eyes of physicists. The theory of relativity was proving not virile but degenerate; it was losing its "first fine careless rapture" in a sandy desert of incongruities; and I do not think it has ever recovered.

There was, of course, a certain solid physical content to these unsatisfactory theories. In a paper published in the *Quarterly Journal of Mathematics* (Oxford) for 1934, I showed that the Einstein-de Sitter solution (flat, expanding space) had an exact counterpart in a Newtonian expanding universe in which every particle possessed just the parabolic velocity of escape from every other particle. The equations on the two views were

identical: *either*, one could use the complicated equations of general relativity to obtain the equations of motion; *or*, one could take a homogeneous Newtonian universe, of infinite extent, in which every particle was separating from every other with just the parabolic velocity corresponding to the attraction of the matter between the two, and derive the equations of motion by applying classical Newtonian mechanics and gravitation together with the equation of hydrodynamical continuity. These considerations yielded a velocity-distance law of recession of the observed type. In other words, Newton himself, without relativity at his disposal at all, could easily have handled the problem of the expanding universe within the limitations inherent in his idea of time!

After that, McCrea and I followed up the matter by considering the cases of elliptic and hyperbolic motions. We showed that the analysis for the elliptic case corresponded to universes of positive curvature, capable of undergoing oscillations corresponding to the librations of elliptic motion; and that the analysis for the hyperbolic case (velocity exceeding the escape-velocity) corresponded to expanding universes of negative curvature. The curvature, in the relativity solutions, could be expressed in terms of the excess of the square of the velocity of the receding galaxy over the square of the parabolic velocity of escape at that point. It was not merely a case of correspondence: the two pieces of analysis were identical in their governing equations. The space used in the Newtonian analysis was ordinary flat space. This showed once again that Einstein's relativity is in no sense dealing with some supposed entity, "curved space", but that it merely constructs a map, in some unusual space, of a phenomenon which may equally well be described in terms of flat space. These Newtonian solutions gave the same velocity-distance law as the relativity solutions, and the same relation between local density and local time. What the Newtonian solutions did *not* do was to give conditions at great distances in satisfactory form. For conditions at great distances must be compatible with the exclusion of velocities exceeding that of light. The simple proportionality of velocity to distance eventually violates this condition in an infinite Newtonian expanding universe. This is connected with the failure of Newtonian relativity to give a satisfactory meaning to simultaneity. But the way of escape of the relativistic expanding universes is also unsatisfactory: where velocities exceeding that of light would be found in the Newtonian solution, there the relativity solutions just refuse to play! They behave as though the material beyond the critical distance does not exist until required by the expansion of the critical distance; and then they create the matter as it is required, or annihilate it as it ceases to be required in the contracting case.

In effect, the solutions derived from Einstein's theory afford no superiority over the Newtonian solutions; they are an improvement on the Newtonian corresponding solutions only by a piece of scientific legerdemain. The analysis is out of control.

Of course, almost every theory, however unsatisfactory in its remoter consequences, is capable of teaching us something. What the work on Newtonian expanding universes has taught us is the *relativity of acceleration* in the Newtonian scheme; it has freed the Newtonian scheme from the concept of *absolute acceleration*. It has always seemed odd that Newtonian mechanics, whilst not caring about the velocity of a frame of reference, cared acutely about its acceleration. This is now seen to be due to the use made in Newtonian mechanics of the concept of empty space, which the study of galaxies in the large shows to be an illegitimate concept: we can never ignore the gravitational pull of the "rest of the universe", but the value of this depends on the galaxy taken as frame of reference. When Newtonian mechanics and gravitation are being adopted, account must be taken of the *relative* accelerations of the galaxies with respect to one another. The important point now emerges that if we consider two galaxies O and O′, and a third distant particle P, then consistent results are obtained for the acceleration of P whichever of O and O′ is taken as having zero acceleration. That is to say, if we calculate the acceleration of P relative to O as due to the matter included in the sphere of centre O and radius OP, O being taken to have zero acceleration; if we then determine the

acceleration of O′ as due to the matter in the sphere of centre O and radius OO′; and if lastly we transform the acceleration of P so as to obtain it relative to O′ allowing for the acceleration of O′ relative to O—we get the same result as if we had assumed O′ to have zero acceleration and calculated directly the acceleration of P relative to it, from the matter in the sphere O′P. In other words, a galaxy O′ has no absolute acceleration within the framework of Newtonian mechanics; its acceleration is arbitrary depending on the galaxy O used as origin of reference. This relativity of Newtonian mechanics with regard to acceleration, this denial of the existence of an absolute acceleration when cosmological considerations are taken into account, is intimately bound up with the forms of the Newtonian law of gravitation and the laws of motion. Indeed, it would not be a difficult task to *deduce* the Newtonian law of gravitation from the principle of the relativity of acceleration and the laws of motion. This shows that though in empty space Newton had to posit the laws of motion and the law of gravitation as separate propositions, when account is taken of the way space is filled up, the law of gravitation is not a separate proposition but is deducible from the laws of motion.

In short, as regards its formal consistency with world-structure, Newtonian mechanics is more successful than it had any *a priori* right to be. Its formal content is the same as the formal content of the expanding space theories. Like them it comes to grief chiefly through conditions at great distances. How are we to construct a scheme of world-structure which saves what is good in the Newtonian scheme, and at the same time reconciles the existence of an unbounded universe and the impossibility of material velocities exceeding the speed of light with a velocity-distance proportionality and a universe homogeneous in the large, at the same time preserving the fundamental ideas of relativity?

In my researches since 1932, I consider that I have gone a great way towards solving this problem. I have used markedly unconventional methods, and obtained markedly unconventional results, but to my mind the results go far not only towards removing the difficulties of a physical character that had surrounded the problem of world-structure, but also towards giving answers to some of the deeper philosophical questions that arise as to the nature of gravitation and laws of motion, the scale of time and creation in time, and the ultimate history of the universe. My papers have not been purely or even chiefly astronomical, and they have been published in a variety of journals as opportunity offered. I have never yet formally presented the results of kinematic relativity to this, the Royal Astronomical Society. This was not out of any lack of respect to the Society. It was rather that the earlier papers were of so experimental a character, in the sense of experimenting with ideas, that I had not initially absolute confidence that the general lines of argumentation were sound; moreover, the various results were so novel that one had no familiar physical landmarks to guide one, and neither could outright mistakes be properly guarded against nor could unconscious strayings from the path of the true line of development be avoided. Under those circumstances I would not ask this Society to publish my work, nor did I wish to have to expend energy in replying to the misunderstandings which public debate of not fully worked-out ideas would then have involved. Now that I have confidence in the general correctness of my results, now that I have had time to subject every result to intense internal criticism, now that I can recognize the main line of development and relate it to Newtonian ideas, I take the opportunity of the Presidential Address to give to the Society a general account of the work which has been my inward joy for the past twelve years. I can do no less for a Society to which I owe so much.

I must first explain that it is possible to attack a new field on different *axiomatic levels*. Much of the criticism which kinematic relativity has evoked has been due to misunderstandings which I must clear away at the outset.

A finished discipline usually rests on foundations at a single axiomatic level. Examples of such are (1) Euclidean geometry; (2) Newtonian mechanics and gravitation; (3)

thermodynamics; (4) Einstein's relativity of uniform motion; (5) Einstein's theory of gravitation. But when one starts a new field, one is unaware on what level to start, and on different occasions one can experiment with different sets of assumptions, *i.e.* at different axiomatic levels. Kinematic relativity divides itself into two main branches, one reposing on the Lorentz formulæ, the other an attempt to see on what the Lorentz formulæ themselves repose. Let me go into some detail.

Euclidean geometry rests on a famous set of axioms, amongst them the axiom of parallels, and much research has been devoted to reducing these axioms in number, that is, to deepening the axiomatic level. Research has gone in the direction of erecting the same structure on a smaller foundation, as well as in extending the structure itself. The most notable of these attempts have consisted in tracing the consequences of denying the axiom of parallels in its Euclidean form. The structures thus erected are called non-Euclidean geometries, Lobatschewskian or Riemannian as the case may be. Some may imagine that having spoken with disfavour of spherical space in the earlier parts of this address, I have now no right to speak approvingly of non-Euclidean geometries; in defence I should mention that it is known that Riemannian geometry of two dimensions is equivalent to Euclidean geometry on the surface of a sphere in three dimensions, whilst Lobatschewskian geometry is equivalent to Euclidean geometry on a certain surface of negative curvature; non-Euclidean two-dimensional geometries have counterparts in nature because nature provides a third spatial dimension. But my point here is that those who are antipathetic to the project of narrowing down an axiomatic foundation until the discipline in question becomes wholly or almost wholly deductive, tend to forget the harvest reaped from this procedure in the past, and even use this harvest, on the other prong of the harvester's fork, when they brandish it in criticism of kinematic relativity, when they criticise my procedure of selecting a *flat* private space for each observer and assert that in the presence of gravitation space *must* be curved.

Until relativity came along, mechanics had done little to investigate in similar fashion its own axiomatic basis, namely the laws of motion plus the law of gravitation. Generalisations indeed there were, notably the grand law of the conservation of energy, the momentum laws and the principle of least action. Certain combinations of these could be used as alternative bases, but it was not clear that they were more primitive, that they assumed less. Again, thermodynamics, as resting on its two empirical laws, was transformed into statistical mechanics, but the basis of the latter remained Newtonian. The next great step was taken by Einstein, who by adding a new optical postulate, the constancy of the speed of light to all observers in uniform relative motion, solved the paradoxes arising from the failures to determine an absolute velocity through the æther, analysed the concept of simultaneity and the principles behind the compounding of velocities, and unified mass and energy, dynamics and electrodynamics, in masterly fashion. This phase of Einstein's work, I consider, was in the true line of descent of classical mathematical physics. The invariance of Maxwell's equations, the modification of Newton's equations of motion to make them applicable to high velocities, were established by means of that marvellous weapon, the group of operations called the Lorentz formulæ, now deduced by Einstein not on the cumbrous basis of the Michelson-Morley experiment, the assumed Lorentz-Fitzgerald contraction, or Larmor and Lorentz's treatment of electromagnetic equations, but by simple appeal to his postulate of the invariance of the speed of light to all observers in uniform relative motion.

There was, however, one apparent blemish in this structure: gravitation obstinately refused to come within the scope of the Lorentz formulæ. The famous inverse square law of attraction, $\gamma m_1 m_2/r^2$, or the potential of a single point-mass, $\gamma m/r$, could not be made amenable to the Lorentz transformation. Einstein, misled by the success of his invariance formula $ds^2 = c^2dt^2 - \Sigma dx^2$, which is essentially a deductive consequence of the Lorentz formulæ, not a primitive statement, changed his ground; abandoning the main

line of descent of classical mathematical physics, he retained the notion of an interval but altered its analytical form. He abandoned the Minkowskian time-space in favour of mathematical statements containing undefined symbols, forgetting that co-ordinates can only be ascertained by observers who must be introduced first. In doing so, he to my mind fell heavily into the pit out of which he had so brilliantly climbed in his earlier work, when he insisted that it was illegitimate to introduce any symbol until its value could be ascertained in terms of operations that could actually be carried out.

It can be seen now that Einstein's failure to exhibit the inverse square law of gravitation in Lorentz invariant form was due to his having taken over from the physicist, unanalysed, the notion of a uniform clock and the notion of an unalterable length-measure. When we dissect the idea of a uniform clock, we find that it stands for two distinct modes of measuring time, one appropriate to the Lorentz formulæ and electromagnetic phenomena, the other appropriate to mechanics and gravitation. When we use the right scale of time, there is no difficulty in expressing the inverse square law in Lorentz invariant form. Newtonian time is not what it seems. When we have made clear this dichotomy of time-measurement, the cosmological problem stands solved. What was wrong with Einstein's procedure was his starting from an arbitrary postulate.

My own programme, the results of which I have thus indicated, was executed by following Einstein's own maxim, of never introducing a symbol that could not be operationally defined. But the programme was not executed all at once, and the successive stages through which the investigations have progressed correspond to attacks on different axiomatic levels. I am conscious of having made developments at four such levels. All were inspired by the hope of understanding the expanding universe of galaxies, and by the promise of a deeper understanding of gravitation.

I cannot, in this address, go into many technical details. For them I refer readers to the papers mentioned in the bibliography at the end. But I can attempt to expose some of the complexes of ideas concerned.

The first axiomatic level was a very simple one: the consideration of a cluster of uniformly moving particles in unbounded Euclidean space. These I showed would eventually separate out in velocity, and obey a velocity-distance proportionality. This picture was of course intended to prescribe ideas for development. It has been stupidly criticised on the ground that I "neglected gravitation". The whole art of scientific advance, on the contrary, consists in recognising the essential features of a situation and ignoring the rest. My original picture-model showed that something not gravitation, something I will call "kinematics", was responsible for the observed velocity-segregation —was the essential feature of the situation.

The second axiomatic level was reached when I imposed on my cluster of particles the condition that the distribution of relative velocities, as judged by an observer attached to any particle of the system, should have no preferential features, *i.e.* should be described statistically in the same way from particle-observer to particle-observer. This was the most natural condition in the world to impose, and I am still amazed at the outcry it caused. Einstein had imposed the condition of world-homogeneity without a murmur of disapprobation. Yet when I attempted to find, and, moreover, succeeded in finding, a velocity-distribution for my cluster which put all members of the cluster on the same footing, I was accused of abandoning the scientific method, of imposing a form on the world with a teleological object. Naturally the so-called cosmological principle was merely a definition of systems proposed for study, and their study and subsequent comparison with the world of nature was a perfectly legitimate procedure. Perhaps I was ill-advised to use the term "cosmological principle", but if I have outgrown it I do not regret it. It was good for men of science to be temporarily shocked!

In dealing with the relative-velocity distribution in the cluster, I had to use a formula for the relative velocity of two particles whose velocities relative to the observer were

given. Naturally I used the Einstein formula for relative velocity, which is a simple deduction from the Lorentz formulæ. I obtained as a result the velocity-distribution

$$\frac{Bdudvdw}{c^3[1-(u^2+v^2+w^2)/c^2]^2}. \tag{1}$$

A host of questions now clamoured for answer: What was the ultimate spatio-temporal distribution of the cluster? Would the particles actually continue to move in the uniformly described straight lines that had been assumed at axiomatic level I? What would be the motion of an additional free particle at large amongst the cluster? What was the bearing of this free motion to gravitation as normally understood?

All these questions proved capable of answer at axiomatic level II—the level of the Lorentz formulæ.

From axiomatic level I, the ultimate positions (x, y, z) at time t of members of the cluster, reckoned from any one member taken as origin, clearly obeyed the asymptotic relations

$$\frac{x}{t} \sim u, \qquad \frac{y}{t} \sim v, \qquad \frac{z}{t} \sim w.$$

This meant that the density-distribution n asymptotically approached the spatio-temporal law

$$ndxdydz = \frac{Btdxdydz}{c^3(t^2-\Sigma x^2/c^2)^2}, \tag{2}$$

at any instant t, where t is reckoned from the epoch of the initial configuration of the cluster. It now suggests itself that we consider a cluster formally described by the law (2), on its merits, entirely independently of the path by which we have reached it. The associated velocity-law now becomes precisely

$$u = \frac{x}{t}, \qquad v = \frac{y}{t}, \qquad w = \frac{z}{t}, \tag{3}$$

and (2) and (3) together satisfy the hydrodynamical equation of continuity

$$\frac{\partial n}{\partial t} + \frac{\partial (nu)}{\partial x} + \frac{\partial (nv)}{\partial y} + \frac{\partial (nw)}{\partial z} = 0. \tag{4}$$

The cluster is therefore a legitimate self-consistent object for study.

But it is an entity far more significant than any other hydrodynamical cluster of moving particles that could be suggested. For it has the magnificent property that the statistical description of the system in space, time and velocity—the historical development of the system—is the same from whatever particle of the system it is reckoned. In particular, since the system is symmetrical about the particle at the origin, it is symmetrical about every other particle taken as origin, and thus every particle is itself equally the centre of the system, in its own reckoning of time and space. These are consequences of the Lorentz formulæ. The time $t=0$ is a singular epoch in the history of the system, and this singularity, propagated outwards with speed c, repeats itself over the expanding boundary $r=ct$. But just as the singular epoch $t=0$ is inaccessible as we trace history backwards, so the boundary $r=ct$ is inaccessible to any material particle (not necessarily one of its members) projected within the system. The accessible members of the cluster are therefore confined to the *interior* of the sphere $r=ct$, and thus form an "open" set of particles, in the language of sets of points. To any particle inhabiting it, therefore, the cluster has all the properties of homogeneous, infinite space. It has in fact all the desirable properties of a model for the expanding universe, without any of the paradoxes or incongruities that the curved space solutions involved.

It forms an expanding system, with a strict velocity-distance proportionality in the private Euclidean space of any chosen particle-observer of the system. Particle-number in any closed surface consisting of particles is conserved, and there is no creation or annihilation of matter at any accessible epoch. The system does not exist for negative values of t, and so it is necessarily in a state of expansion; contraction or oscillation is not possible. The system comes into existence at $t=0$, the natural origin of time, and we can if we like call this epoch "creation". This epoch repeats itself at the inaccessible expanding frontier $r=ct$, near which events reckoned by us as contemporary with ourselves are reckoned by the observers experiencing them as very early in time. As regards the density-singularity at any point of the boundary $r=ct$, this is just what is required to give satisfactory boundary conditions. It means that to immensely powerful telescopes viewing the system from the origin, the particles near the boundary would be indefinitely close together, and owing to their recession with speeds near that of light, indefinitely faint. There would be no "holes" in the background of particles, neither would it be, as in the Newtonian static universe, indefinitely bright. There is of course no difficulty about accommodating the crowded population of galaxies near the expanding frontier, for to an observer amongst them the situation is no whit different from the situation at the same local epoch anywhere else, and to an observer distant from them they are moving with speeds so near that of light that the enormous Lorentz-Fitzgerald contraction in the line of sight leaves the required room.

To ask what is beyond the boundary is an illegitimate question. It is like asking "What was?" before creation took place. To any observer attached to any particle of the system, the finite but increasing domain of space required by the system is brought into existence by the system's own expansion. To ask what is the experience of a particle at the boundary is equally illegitimate; it is like asking for the experience of a particle at the epoch of its creation. It must *have been* created before it can have any experiences to chronicle.

It must be now clear that whether the actual galaxies follow this system of motions on the average or not, we have here a system of frames of reference so related that they bring in the essential features of possessing synchronised and rated time-measures (in virtue of having parted company at a common event), and an essentially *forward* march of time, which satisfy proper boundary conditions, and which are therefore capable of acting as a background or reference-system for phenomena in general. This system, which elsewhere I have called the *substratum*, is the clean slate on which the pattern of phenomena is to be drawn. It plays the same part in relation to phenomena as the two-dimensional plane of Euclid plays in relation to the static figures of geometry. It is available as the scene, the theatre, of events dynamical, optical, gravitational, electromagnetic. Its hierarchical position is superior to the Euclidean plane, in that it incorporates, as essential features in its construction, the idea of motion and the one-sensed flow of time.

Maintaining the same axiomatic level, *i.e.* continuing to assume the Lorentz formulæ, it is possible now to go forward, and explore the dynamical, gravitational and electromagnetic properties of events taking place in the substratum. It is first shown that the substratum is a *natural* system; that is, that once in existence it will continue of itself. For every particle being central in its own frame of reference, by symmetry it will continue central. More satisfactorily the question can be answered by ascertaining the equation of motion of an additional free particle liberated or projected inside the system. This is a long and difficult investigation, and involves the construction of systems more general than a substratum and the discussion of their motion. But it can be shown at an early stage of this investigation that when the free particle is reduced to coincide momentarily in position and velocity with one of the original particles of the cluster, it does so always, thus showing that an original particle behaves as if free. The original particles are called the *fundamental* particles of the system.

On the basis of the equation of motion of a free particle, we can distinguish between free and constrained particles, and so introduce a measure of force. That done, we can construct a dynamics, and ascertain the analogues of mass, work, energy, momentum, and establish the equivalence of mass and energy. Throughout this investigation we assume no empirical physics, save only the Lorentz formulæ; in particular we do not assume Newton's laws of motion.

The resulting dynamics is strangely unlike both Newton's dynamics and Einstein's dynamics: the acceleration of a free particle at a given position, epoch and velocity with respect to the observer, instead of being zero, comes out as a function of that position, epoch and velocity, whose form is of course the same whatever fundamental particle is taken as origin; it thus expresses the gravitational relation between the free particle and the whole substratum. This relation, as do all the other dynamical quantities occurring in the theory, involves t, the epoch or present age of the system to the observer concerned—an entirely new feature in dynamics. For a particle at rest at a cosmically small distance r from the observer, the free acceleration is approximately $-r/t^2$, which be it noted is quite free from arbitrary constants.

This is the simplest expression of gravitation in a locally homogeneous system. To compare it with the Newtonian description of gravitation, we note that the particle density in the vicinity of the observer is, by (2), B/c^3t^3; if we give each particle a Newtonian mass m, the Newtonian attracting mass inside the sphere, centre the observer, extending to the attracted particle, is $\frac{4}{3}\pi r^3 mB/c^3t^3$; its Newtonian attraction is accordingly $-\frac{4}{3}\pi\gamma rmB/c^3t^3$; and if we identify this with the kinematically determined acceleration $-r/t^2$ we get $\gamma = c^3t/\frac{4}{3}\pi mB$. But $\frac{4}{3}\pi mB$ is the mass that would fill the sphere of radius ct with matter of the local density mB/c^3t^3. Call this mass M. Then $\gamma = c^3t/M$. This expression, if we force a Newtonian interpretation on our kinematics, gives the value of the Newtonian "constant" of gravitation in a form involving c, the speed of light, t, the present age of the universe (to ourselves), and M, the mass of the equivalent homogeneous universe. As I have shown elsewhere, this agrees with the observed value. The apparently strange circumstance that γ involves t, and so is not strictly constant, is accounted for later.

The foregoing is merely an elementary simplification of arguments of a much more precise nature, which go much deeper. We can go further still, and obtain the joint potential energy χ of two massive particles, m_1, m_2, in one another's presence and the presence of the substratum, in the form

$$\chi = -\frac{m_1 m_2 c^2}{M}\frac{X_{12}}{[X_{12}^2 - X_1 X_2]^{\frac{1}{2}}}, \tag{5}$$

where

$$X_1 = t_1^2 - P_1^2/c^2, \qquad X_2 = t_2^2 - P_2^2/c^2, \qquad X_{12} = t_1 t_2 - P_1 P_2/c^2,$$

and (t_1, P_1), (t_2, P_2) are the epochs and positions of the particles considered, in the reckoning of the observer concerned. When we choose the observer to be at one of the particles concerned, say m_2, then $P_2 = 0$, and χ reduces to

$$\chi = -\frac{m_1 m_2 c^3 t_1}{M}\frac{1}{|P_1|} = -\frac{\gamma m_1 m_2}{r}. \tag{6}$$

Now (5) is invariant under a Lorentz transformation from any one fundamental particle-observer in the substratum to any other. Since it reduces to a multiple of $1/r$ when $P_2 = 0$, it is the desired relativistic expression of $1/r$ in Lorentz-invariant form.

We have achieved what Einstein, Poincaré and others deemed impossible because we have not imposed the empirical Newtonian formulation of gravitation *a priori*, but because we have sought the underlying kinematic basis of gravitation, and found it to vary with epoch.

We have had no need for a *theory* of gravitation in our kinematic relativity of the expanding universe. We have made no hypotheses about gravitation; the desired quantitative formulation imposed itself. Whether expression (5) yields the finer details of gravitational phenomena as observed not from a fundamental particle but from a moving planet in a moving solar system remains for future investigation; it must be remembered that (5) refers to observations of co-ordinates made from a *fundamental* particle. It may require refinement in the future. But I am convinced that the main line of argumentation holds good. There is no *force* of gravitation in the sense of æthereal tubes of force, no curvature of space *imposed by* gravitation ((5) refers to observers' private flat spaces); there is merely a description of the inevitable behaviour of two particles in one another's presence in order that different fundamental observers may make consistent observations on them. Kinematic theory is the ultimate statement of metaphysical refinement, in which every gross influence, whether of æther, space or tube of force, is purged away, and mere existence and motion remain.

But when we have accounted for the expanding universe and the velocity-distance law; when we have shown that our model is a *natural* system; when we have found the motion of a free particle and erected thereon a dynamics and identified mass, work, energy and momentum; when we have extended the investigations to cover gravitation and electrodynamics—we still have not finished with the possibilities of the situation. So far we have worked on axiomatic level II, the level of the Lorentz formulæ. Can we go deeper, work backwards to something more primitive? Just as in Euclidean geometry we can either go forward and establish fresh theorems or go backwards and reduce the axioms, so here we can attempt a "proof" of the Lorentz formulæ more primitive than Einstein's. There is no obligation to do this as far as physics is concerned; the forward march of the argument suffices. But it is fascinating to proceed now to axiomatic level III, which is, namely, the deduction of the Lorentz formulæ on the sole basis of the observers' consciousness of the passage of time. We are to have at each observer's disposal a single measuring instrument, the clock, with which he can determine co-ordinates, epochs and velocities, and so by comparing his observations with those of other observers in uniform relative motion provided with synchronised and rated clocks, inquire whether the relations between the measures involved obey the Lorentz formulæ. We are to dispense completely with Einstein's empirical postulate of invariant light-speed, because until we have analysed our observers' observations we know nothing about the speed of light, or even how to describe it; we simply fix our length measures in terms of light-transmission experiments, in which we assume nothing whatever about the speed of light.

Perhaps nothing in my investigations has been more gratuitously misunderstood than this research at the more primitive level III. I had no need to undertake it; I could have rested quite content with Einstein's basis for the Lorentz formulæ. But who could be satisfied with an arbitrary assumption when so much of consequence followed? It was essential to see what underlay the Lorentz formulæ, and how deep we could get below them.

I cannot go into the details here. An account of the investigations will be found in the earlier parts of my book on *World-Structure* of 1935, wherein, to be logical, I put the derivation of the Lorentz formulæ on a time-basis *first*, although actually this derivation was only made after the main kinematic structure was completed. But the sequel is that the Lorentz formulæ can be arrived at by simply writing down the observations the observers must make if they are to be consistent one with another, provided they have verified by means of the same clocks that their relative velocity is uniform. The Lorentz formulæ do not depend on any quantitative property of light whatever. Just as gravitation depends on nothing of a kind that might be called gravitational, so the Lorentz formulæ depend on nothing of a kind that might be called optical. In investigating the Lorentz formulæ one as it were consumes all one's own ashes, until

nothing empirical remains; the only conditions are ontological—that the things talked of can be the objects of perception.

Many physicists dislike this state of affairs. Without an empirical basis they feel rudderless and havenless. I cannot help them—the state of affairs is not my fault. Those who criticise the position my work has compelled me to adopt should not rest content with mere dissatisfaction: they should point out where unsuspected empirical assumptions enter into the various stages of the investigation. I repeat: the Lorentz formulæ are merely the consequences of our being able to perceive the external world.

This may seem to have taken us far from cosmology. Let me show its connection with the expanding universe. Having achieved progress at axiomatic level III, we can attempt to go still deeper. We can try to dispense with our original supposition that the fundamental particles were in *uniform* relative motion. This is possible provided we can generalise the Lorentz formulæ to accommodate non-uniform relative motion. This I carried out in my book *World-Structure*, but I there made no application of the results save to deduce the ordinary Lorentz formulæ as a particular case. But Dr. Whitrow and I have since shown that by regraduating the observers' clocks, it is always possible to reduce any kind of accelerated motion to either uniform motion or to relative rest; the only condition is that when there are three or more observers present, their accelerations must be related in a fashion which is the generalisation of the property of belonging to the class of uniform relative motions.

The whole crux of the matter is, What do we mean by *uniform time*? In the abstract, if two alternative sets of graduations of "equivalent" observers' clocks are considered, there is nothing to choose between them; either may be taken as defining uniform time, and the other is then running either slow or fast. Neither Einstein nor Newton analysed the notion of uniform time. Now the time kept by the Astronomer Royal is the time in which the Newtonian dynamics holds good. Is this the time-scale in which the galaxies are receding? Are they, in this time-scale, uniformly or non-uniformly moving with respect to one another, or even at relative rest? We cannot use the red-shift itself to answer this question, because the interpretation of that will depend on what time-scale the atomic vibrators keep, which is also one of our objects to find out.

Now the Astronomer Royal's time-scale, the Newtonian scale, depends at bottom on the rotation of a rigid body, and the theory of this is built up on the laws of motion. First amongst these is that the free motion of a particle in empty space is uniform. But empty space is a limit, the ideal we should reach at an infinite distance from all matter. In the universe as it is constituted, this ideal is not merely practically but theoretically impossible; there is an upper limit to the distance we can get from any galaxy. We therefore replace the concept of a free particle in empty space by the concept of a free particle in the presence of the galaxies. We have already discussed the form of the equation of motion of such a particle; we found it non-Newtonian, but we could force an interpretation on it in terms of Newtonian gravitation. But we have been accustomed, in Einstein's relativity, to the disappearance of a gravitational field consequent on a change of co-ordinates. Now at axiomatic level IV, co-ordinates are mere combinations of clock-readings. Changes of co-ordinates can be accomplished in many ways; but to throw light on our problem of identifying the scale of time, we must contemplate only changes of co-ordinates which arise by regraduating all observers' clocks in the same way, whilst preserving, to ensure that observers understand one another, the same rules for constructing co-ordinates out of clock-readings.

When we examine the equation of motion of a free particle in the presence of the uniformly expanding universe, we find that there is one particular clock-regraduation which causes the complete disappearance of the free particle's acceleration, reducing it to the uniform motion of Newton's First Law. Moreover, this particular regraduation, from the time t of the Lorentz formulæ to a new time τ, transforms the queer t-dynamics, with its peculiar expressions for mass, energy, momentum, etc., into the corresponding

Newtonian expression with all Einstein's relativistic refinements incorporated. There are, it is true, some minor divergences, and the private flat spaces of the observers using t-time became a public hyperbolic space for the observers using τ-time. But, by and large, the inference is clear. Newtonian time is τ-time. In particular, to clinch matters, angular momentum is conserved in τ-time but not in t-time.

The required clock-regraduation is

$$\tau = t_0 \log (t/t_0) + t_0.$$

The constant t_0 is such as to make t and τ agree at $t = t_0$, and also to make the rates agree at $t = t_0$. We therefore choose t_0 to be the present value of t, *i.e.* the age of the universe on the t-scale. The initial epoch, $t = 0$, now corresponds to $\tau = -\infty$. This expresses in unmistakable form the inaccessibility of the singular epoch through any train of phenomena obeying Newtonian dynamics. Cosmology thus suggests that the world has always *been*, in the sense of Newtonian time, but began a finite time ago ($t_0 = 2 \times 10^9$ years) on the kinematic scale.

As a welcome confirmation of the time-regraduation theory, we find that the description of gravitation now becomes more truly Newtonian, the former γ becoming a constant γ_0 given by $\gamma_0 = c^3 t_0 / M$. Thus the *value* of the "constant" of gravitation still depends on the epoch, but it now behaves as a constant in the equations appropriate to the τ-scale at any particular epoch t_0.

The $t \rightarrow \tau$ regraduation reduces all the fundamental particles of the original substratum, as described in the t-scale, to relative rest, and opens up the finite sphere $r = ct$ so as to embrace the whole of an infinite (hyperbolic) space. The red-shifts are no longer to be interpreted as motion. They are now interpreted as a secular acceleration of atomic frequencies, whereby the older quanta, emitted from galaxies long ago, are slower in frequency than the present terrestrial atoms with which we compare them. Alternatively, it is better to continue to regard the t-scale as the most suitable for optical phenomena, in which case the actual frequency of an atom is constant, and the red-shifts are interpreted as Doppler effects. Newtonian time τ is not the time-variable of the Lorentz formulæ or of Maxwell's equations. These, and with them the electromagnetic theory of light, are most conveniently expressed in terms of the more fundamental, kinematic time t.

I have throughout spoken of the galaxies as particles. But the $t \rightarrow \tau$ transformation is to be expected to throw great light on their spiral structure, inasmuch as gravitational orbits, regarded dynamically as *closed*, will be regarded as *open* on the t-scale.

To conclude, the study of the expanding universe, with the gravitational problems it suggests, has shown that in contemporary physics two time-scales are confused. It was owing to this confusion that gravitational phenomena were thought to be incapable of being brought within the scope of the Lorentz formulæ. It was owing to this confusion that Einstein's relativity was unable to accommodate electromagnetic phenomena in a unified field theory. Though theoretically distinct, the two time-scales coincide for all contemporary purposes. It is only when we trace phenomena to immense distances, or immensely far into the past, that the differences manifest themselves. The disentanglement is made possible by adopting a strictly positivist attitude, by excluding any notion that cannot be defined in terms of the simplest acts of perception. We are led to these results by studying the broadest and most general features of the universe of galaxies as disclosed to observation, and by using the train of thought so started to formulate ideas on the nature of universal gravitation.

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